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ART. I.—*Approximate Cotidal lines of Diurnal and Semi-diurnal Tides of the Coast of the United States on the Gulf of Mexico;* by A. D. BACHE, Superintendent U. S. Coast Survey.

(Communicated by authority of the Treasury Department.)

AT successive meetings of the American Association I have presented approximate cotidal lines for the Atlantic and Pacific Coasts of the United States, drawn from the tidal observations of the Coast Survey. I now present similar lines for our coast on the Gulf of Mexico.

The problem is a very different one from either of the others referred to. The tides on the Atlantic Coast are of the regular semi-diurnal class and easily discussed by the forms already elaborately prepared by Lubbock and Whewell. The diurnal inequality is not large; indeed though easily recognized at particular periods and then quite characteristic, in general it is difficult to trace and often irregular in magnitude and even in sign.

Those of the Pacific Coast are remarkably regular in the semi-diurnal and diurnal waves, and both rise to such heights as to make observation easy. On the Gulf Coast on the contrary the tides are small, and therefore easily influenced by extraneous circumstances, and as a rule, on more than two thirds of the coast the semi-diurnal tides are very small, and in fact are masked by the diurnal tides. The comparatively imperfect discussion which has been made of these tides requires many steps in the discussion to be supplied, and sometimes leaves us in doubt as to the exact interpretation of the results.

By way of preparing for the present discussion and to avoid running into too great length at this time, I gave at the last meeting of the Association an account of the tidal observations made on our Gulf Coast, and showed the type curves for the different stations from Cape Florida to the Rio Grande.* I also explained the method of decomposition of the curves of observation into diurnal and and semi-diurnal waves, and gave the analysis of the type curves at the several tidal stations. From Cape Florida and along the keys, and up the western coast of the peninsula, to St. George's, the tides are of the half day class with a large diurnal inequality; from St. George's which belongs to the day class to South West Pass they are of the day type, the semi-diurnal tide almost disappearing; at Dernière Isle, Calcasieu and Galveston they resume as a rule the half day type, and lose it almost completely at Aransas and the mouth of the Rio Grande. The Dernière Isle and less distinctly, the Calcasieu tides show cases of interference in the semi-diurnal wave, two high waters being at times easily traced in the semi-diurnal curves.

The character of the tidal phenomena themselves, the peculiarities in configuration and in depth of the basin, the limited extent over which our researches spread, and various other circumstances contribute to render this work less satisfactory than the former. Some of these will in the end disappear as the Gulf is more fully explored in the progress of the survey. Our information thus far extends to but one entrance of the basin, that by the Gulf of Florida, and of this to but one shore; while of the nature of the tide wave which enters from the Caribbean Sea through the Straits between the western end of Cuba and the eastern end of Yucatan, we have no reliable information. Some of these causes render general speculation premature, and lie at the very threshold of attempts to trace out the great interference problems which present themselves.

The progress of this discussion has also shown that observations of longer period are necessary in many cases to give data for conclusive results.

This of itself is a great point gained and the practical results for the charts of this coast have themselves repaid all the labor which has been expended on the observation. Navigators were absolutely without information other than the most vague in regard to the tides of the Gulf.

The hourly observations at each station being plotted in diagrams upon a suitable scale, the curves of observation were decomposed by the graphical method introduced by Mr. Pourtales, into a diurnal and semi-diurnal curve. It may be proper to observe here that several comparisons have been made between this method and that which I had formerly used by the sine

* Proceedings Amer. Assoc. Adv. Sc., Providence meeting, 1855, p. 152, and Am. Jour. Sci. and Arts, Jan. 1856, p. 28.

curves and with generally coincident results. The graphical method besides being less laborious is free from the hypothesis of the sine curve. These decompositions were made chiefly by Messrs. Fendall and Heaton of the tidal party, and occasionally by Prof. Pendleton and Mr. S. Walker.

That the diurnal wave is the principal feature, in these tides will appear from the annexed table (No. 1) which gives the

TABLE 1.

Stations.	Height of semi-diurnal tides.	Height of diurnal tides.	Height of observed tides.
	ft.	ft.	ft.
Cape Florida,	1·6	0·2	1·5
Indian Key,	1·9	0·6	1·8
Key West,	1·2	0·7	1·4
Tortugas,	1·0	1·0	1·2
Egmont Key,	1·1	1·6	1·4
Cedar Keys,	2·4	1·5	2·5
St. Marks,	2·2	1·8	2·2
St. George's Island,	0·2	1·6	1·1
Pensacola,	0·2	1·1	1·0
Fort Morgan (Mobile Bay),	0·2	1·1	1·0
Cat Island,	0·3	1·2	1·3
South-West Pass,	0·2	1·2	1·1
Dernière Isle,	0·4	1·6	1·4
Calcasieu,	1·3	1·5	1·1
Galveston,	0·5	1·1	1·6
Aransas,	0·5	1·3	1·1
Brazos Santiago,	0·3	0·8	0·9

names of the tidal stations; the average rise and fall of the tide at each, and the height of the diurnal and semi-diurnal waves composing the observed tides. Table No. 2 shows the

TABLE 2.

Tide stations on the Gulf of Mexico, the results of which are discussed in this paper.

No.	Stations.	Date of Observation.	Kind of gauge.	Observers.
1	Cape Florida, Fla.	April 22, to Oct 31, 1854.	S R.*	L. E. Tansill.
2	Indian Key, "	Jan. 21, to April 16, 1855.	S. R.	do.
3	Key West, "	April 20, 1850, to Dec. 31, 1851.	Box.	W. Lane, and G. W. Goss.
4	Tortugas, "	April 1, to June 2, 1855.	S. R.	C. T. Thompson, and F. Buxton.
5	Egmont Key, "	April 22, to Aug. 23, 1854.	"	C. T. Thompson.
6	Cedar Keys, "	Jan. 10, to March 16, 1852.	Box.	G. Würdeman.
7	St. Marks, "	Nov. 3, 1854, to March 2, 1855.	S. R.	C. T. Thompson.
8	St. Georges Isl "	April 11, to Aug. 16. 1852.	Box.	G. Würdeman.
9	Pensacola, "	Aug. 27, to Oct. 31. 1852.	"	do.
10	Fort Morgan, Ala	May 20, 1850, to May 26, 1851.	"	do.
11	Cat Island, La.	Dec. 29, 1847, to Feb. 13, 1849.	"	G. Würdeman, and R. T. Bassett.
12	South W. Pass, La	Nov. 19, 1852, to Mar 28, 1853.	"	G. Würdeman.
13	Dernière Island, "	April 5, to June 12, 1853.	"	do.
14	Calcasieu, "	Feb. 24, to May 26, 1854.	S. R.	do.
15	Bolivar Point, Tex.	Oct. 1, 1853, to June 1, 1854.	"	L. E. Tansill.
16	Galveston, "	March 21, 1851, to Jan 1, 1853.	Staff.	G. Price and F. Muhr.
17	Aransas Pass, "	Nov. 1, 1853, to Jan 31, 1854.	Box.	G. Würdeman.
18	Braz's Santiago, "	July 7, to Oct. 13, 1853.	"	do.

* Self-registering tide gauge.

the period during which the tidal observations were made, and the names of the observers.

A diagram (No. 1) shows these results graphically. A curved line corresponding to the general outline of the shore cutting off its irregularities, is drawn on the chart of the Gulf Coast, and then developed into a straight line. Thus the tidal stations are plotted at their distances from each other, measured along the general line of the coast. For use by navigators, any intermediate stations may be marked in, in the same way, and a rough approximation to the character of the tide be obtained by the interpolation.

The least observed height is 0·9 feet at Brazos Santiago, and the greatest 2·5 feet at Cedar Keys. The least height of the average semi-diurnal tide is 0·16 feet at South West pass, and the greatest 2·40 feet at Cedar Keys. The least height of the average diurnal tide is 0·21 feet at Cape Florida, and the greatest 1·80 feet at St. Mark's. Of course these numbers are for reasons easily seen only approximations.

As we enter the Gulf of Mexico by the Straits of Florida the height of the tide first increases, then decreases. Passing into the bight at the upper end of the Florida peninsula the rise is greatest. West of St. George's it diminishes, to rise again in the bight formed by the Southern coast of Louisiana and the eastern coast of Texas.

In the decompositions here traced, and in the very laborious discussions tentative and final of the whole of the observations upon which this paper is based, I would acknowledge the great assistance derived from the labors of Assissant L. F. Pourtales in charge of the tidal division of the Coast Survey. The unwearied assiduity of his own labors and his intelligent supervision of the work of others, has been felt at every step in the progress of these investigations. They have required on his part great recources of ingenuity, patience and knowledge.

In discussing semi-diurnal tides, the lunitidal interval of high or low water varying only from a certain mean within moderate limits affords a cardinal datum (the establishment) for the times. In the diurnal tide this datum is wanting. The law of the change of the diurnal tide as expressed in the formula of Prof. Airy (Tides and Waves, Encyc. Metrop., p. 254, Art. 46) is in general represented, but the great flatness in the form of the curves at particular relations of the moon's right ascension and declination, required by the formula does not occur. The general form of these curves is shown upon the diagram No. 2 where the abscissæ represent the days, and the ordinates the lunitidal intervals of high water. About the maximum of declination for some four to six days the lunitidal intervals are moderately constant, and the average of these is what I have

taken for a comparison of the lunitidal intervals to trace the progress of the diurnal wave. The variations from day to day being less than the probable irregularities in the times of high water and the uncertainties in the observations; these means give suitable numbers for comparison. The result would not have been greatly different had only a few of the observations at either end of the declination period been thrown off, but after examination we found these numbers to present apparently the best results.

At four of the stations, namely, Key West, Fort Morgan, Cat Island, and Galveston, hourly observations were continued during a year and upwards, and the decompositions in all the cases but Cat Island embrace that period. The annual change of diurnal establishment is very clearly seen in all these cases and is shown in the diagram No. 3. The law of the change is beautifully developed in the larger tides of the Western Coast and, as deduced from the San Francisco observations, is shown upon the same diagram. In all the cases the actual computed results for the different half-monthly periods are represented by the broken lines on the diagram and the line representing the curve is drawn with a free hand among the points. The general resemblance of these curves with however different maximum ordinates, is very striking, showing that the law of the change is the same, only the coëfficients of the fractions varying.

On the diagram of the San Francisco results the curve derived from Prof. Airy's formula (Theory of tides and waves, Ency. Metrop., p. 254, Art. 46) is drawn as well as that from observation and the general conformity is quite striking.

In making use of the curves as expressing the law of annual change, one of the branches has been turned over upon the other so as to use the mean of the two periods of six months.

At the other fourteen stations on the Gulf of Mexico the observations were continued from one to three lunations, and fell in different parts of the year. To reduce these therefore to the same period of the year it is necessary to employ the data from the localities where the whole annual change was embraced. The results are plotted on the several diagrams, those from the Brazos to South West Pass on the curve from Galveston, those from the South West Pass to St. George's on the curve from Fort Morgan, and those from St. George's to Cape Florida on the curve from Key West. There is, except in one case, a general conformity in the observed changes and in those deduced from the other comparisons, at least there are no greater contradictions than those presented by the observations from which the mean curves are drawn. From these plottings the correction necessary to reduce the result to the mean of the year are deduced and the annexed table shows the diurnal interval as deduced directly from the ob-

servations and as corrected. It is satisfactory to see that the correction makes the results more conformable to law, increasing therefore the probability that the correction is rightly applied and is approximately correct in magnitude.

TABLE 3.

	Stations.	Lat.	Long.	Long. in time.	Mean diurnal Lunitidal interv'l near max. of moon's Dec.	Sum of Long. and Estab.	Correc. for transit.	Correc. for depth.	Correction for yearly variat'n of diurnal lunit. interval.	Corr'ted cot. hour.
		° ′	° ′	h. m.	h. m.	m.			h. m.	h. m.
1	Cape Florida,...	25 40	80 9	5 21	14 27	19 48	0 27	0 20	+ 0 18	19 19
2	Indian Key,....	24 52	80 44	5 23	17 00	22 23	34	20	− 0 42	20 47
3	Key West,.....	24 27	81 53	5 28	18 32	24 0	37	19	0 0	23 4
4	Tortugas,.....	24 34	82 59	5 32	18 5	23 37	36	10	+ 0 43	24 34
5	Egmont Key,...	27 36	82 46	5 31	19 19	24 50	39	20	+ 0 1	23 52
6	Cedar Key,.....	28 58	82 57	5 32	22 27	27 59	43	45	− 0 57	25 34
7	St Mark's,.....	30 0	84 11	5 37	21 56	27 33	42	22	+ 0 20	26 49
8	St. George's Isl'd.	29 35	85 12	5 41	19 41	25 22	42	25	+ 0 37	24 52
9	Pensacola,.....	30 18	87 15	5 49	21 28	27 17	42	17	+ 0 22	26 40
10	Fort Morgan,...	30 9	88 0	5 52	21 21	27 13	42	14	0 0	26 14
11	Cat Island,.....	30 6	88 38	5 55	22 21	28 16	43	1 21	− 0 11	26 1
12	South West Pass.	28 56	89 22	5 57	19 28	25 25	39	21	+ 0 59	25 24
13	Dernière Isle,...	28 58	90 58	6 4	19 19	25 23	38	24	+ 1 37	25 58
14	Calcasieu,.....	29 40	93 21	6 13	19 22	25 55	39	17	+ 1 27	26 6
15	Bolivar Point,...	29 23	94 46	6 19	21 40	27 59	43	12	+ 1 24	28 28
16	Galveston,.....	29 18	94 41	6 19	22 29	28 48	43	30	0 0	27 35
17	Aransas Pass,..	28 15	96 31	6 26	19 46	26 12	40	17	+ 0 43	26 0
18	Brazos Santiago	26 6	97 10	6 29	20 21	26 50	41	15	− 1 39	25 16

The first column of the annexed table No. 3 contains a number for reference, the second, the name of the tidal station, the third, fourth, and fifth, the latitude and longitude, the latter in degrees and in time, the sixth, the lunitidal interval about the maximum of declination, the seventh, the sum of this last named number and the longitude in time, the eighth, the correction to reduce the observations to the same transit, the ninth, the correction for depth, carrying them by the law of depth to deep water, the tenth, the correction to reduce the observed lunitidal interval at maximum to the corresponding mean of the year, the eleventh, the corrected cotidal hour.

The table enables us satisfactorily to trace the diurnal wave from Cape Florida to the Tortugas, across by the deep water of the Gulf to South West Pass at the entrance of the Mississippi, and from this line of deep water to the Western Coast of the peninsula of Florida by Egmont Key (Tampa), Cedar Keys, St. Marks and St. George's, and in the bay between South West Pass and St. George's by Cat Island, Fort Morgan, Pensacola, and St. George's. Again in the bight between East Bayou and the Rio Grande, to Isle Dernière, the Rio Grande, Calcasieu, and Aransas up to Galveston. In obtaining the general direction of the cotidal lines, I have followed the method of grouping used in

my former papers in the form given by Professor Lloyd. It is easy to obtain a general view of the movement of the diurnal wave in this way, but the selection of the groups required a tedious set of trials and the discussion of many groups which appeared natural, proved very unsatisfactorily. The burthen of the computation for this work has fallen upon Mr. John Downes.

Table No. 4 shows the groups selected, with a letter attached for reference, the names of the stations constituting the groups, the mean latitude and longitude, and cotidal hour of the group, the values of the coefficients of each, the angle of the cotidal line with the meridian, the velocity of movement of the wave, and the same in miles per hour.

TABLE 4.

Stations.	Mean Longitude.	Mean Latitude.	Mean cotidal hour.	M Diff. of cotidal hour for one geog. mile of Longit'e.	N Diff. of cotidal hour for one geog. mile of Latitude.	Angle tang $\frac{N}{M}$ Co-tidal angle.	Diff. of cotidal hour corresponding to one geog. mile perpend. to cotidal line $\frac{1}{\sqrt{M^2+N^2}}$	Miles per h. tidal wave.
	° ′	° ′	h. m.			° ′		
A Cape Florida, Indian Key, Key West.	80 55	25 0	21 3	−1·978	−0·509	−14 28	2·052	29·2
B Indian Key, Key West, Tortugas.	81 52	24 38	22 28	−0·890	−3·240	74 38	3·360	17·8
C Key West, Egmont Key, Cedar Keys.	82 32	27 0	24 10	5·251	1·695	−17 53	5·518	10·9
D Cedar Keys, St. Marks, St. Georges.	84 7	29 31	25 45	1·095	2·358	−65 5	2·600	22·7
E St. George, Pensacola, Ft. Morgan.	86 49	30 1	25 55	0·055	2·645	−88 49	2·645	22·7
F Cedar Key, St. Marks, St. George's, Pensacola, Ft. Morgan, Cat Island, S. West Pass.	86 31	29 43	25 56	0·048	0·911	−86 58	0·910	66·0
G St. Georges, Pensacola, Ft Morgan, Cat Isl'd, South West Pass.	87 41	29 49	25 50	−0·275	1·092	75 52	1·127	53·1
H South West Pass, Dernière Isl'd, Calcasieu, Galveston, Aransas, Brazos.	93 45	28 28	26 3	−0·241	+0·944	63 19	0·974	61·6

On the character of these groups I would remark as follows. Groups A, Cape Florida, Indian Key, and Key West, and B, Indian Key, Key West, and Tortugas, give a natural movement for that of the wave, though showing a more abrupt change than is probably real. The computed and observed cotidal hours differ at the greatest but one minute and a quarter. The next group C, gives a satisfactory idea of the movement of the wave passing round the Tortugas and up along the coast of the peninsula, over the extensive flat which borders it. The next group D, Cedar Keys, St. Marks, and St. George's, presents a perfect agreement between the computed and observed cotidal hours, and a direction and velocity agreeing with what might have been sup-

posed. The same is true for group E, St. George's, Pensacola, and Fort Morgan. The more general group F, including the stations from Cedar Keys to South West Pass agrees in its indications with those given by the partial groups, as does also G, including the stations from St. George's to South West Pass. In passing westward and southward the direction of the line changes rapidly and no satisfactory adjustment by groups could be made. From South West Pass to Brazos Santiago the smaller groups give decidedly anomalous results for adjacent stations, pointing to the more general arrangement of the line shown by group H, composed of South West Pass, Dernière Isle, Calcasieu, Galveston, Aransas, and Brazos Santiago.

The agreement of the cotidal hours as computed and observed is less satisfactory in the larger groups than in the smaller ones as might be expected. On the map No. 5, a rough outline of the Gulf Coast is traced, and the cotidal hours are marked near the stations. The mean cotidal line for each group and the hour before and after the mean hour are marked on the map, showing the direction and velocity of the diurnal wave as given by the groups. From a consideration of these and of their necessary connection the cotidal lines are approximately drawn. The great cotidal line of the Gulf as traced upon the chart is that of twenty five hours.

The cotidal lines of 19 to 23 hours only appear on the coast of the Florida Keys. The line of 24 hours is well marked near Egmont Key (Tampa). The line of 26 hours is at the head of the bight between St. George's and Cedar Keys, and in that near Cat Island. The line of 27 hours appears only at the head of the bay formed by the coasts of Texas and Louisiana.

The table No. 5 of semi-diurnal tides is in the same form as No. 3 for diurnal tides. It contains a number for reference, the name of the station, its latitude, the longitude in arc and in time, the establishment, the same in Greenwich time, the correction for transit and for depth, and the corrected cotidal hour.

In tracing the semi-diurnal wave as it enters the Straits of Florida we find after a slight contradiction between Cape Florida and Indian Key, a general increase of the cotidal hour in the right direction to the Tortugas. The semi-diurnal wave here gives a difference of time between Cape Florida and the Tortugas of but 1^h, 24^m, while the diurnal wave gave 4^h, 03^m. The lagging of the diurnal wave which at Cape Florida was 1^h, 44^m, at Indian Key is 3^h, 22^m, at Key West 4^h, 31^m, and at the Tortugas 4^h, 23^m.

The semi-diurnal wave passes across the Gulf to the South West Pass as did the diurnal. The time of crossing by the semi-diurnal wave is, however, 1^h, 13^m, while by the diurnal wave it was 1^h, 50^m.

TABLE 5.

	Stations.	Lat.	Longitude in arc.	Longitude in time	Est'blishment of half daily tide.	Establishment Gr'uwi'h time.	Correction for transit.	Correction for depth.	Corr'ected cotidal hour.
		° ′	° ′	h. m.	h. m.	h. m.	m.	m.	h. m.
1	Cape Florida,	25 40	80 9	5 21	8 17	13 38	− 17	− 20	13 1
2	Indian Key,	24 52	80 44	5 23	8 2	13 25	− 16	− 15	12 54
3	Key West,	24 27	81 53	5 28	9 10	14 38	− 18	− 19	14 1
4	Tortugas,	24 34	82 59	5 32	9 22	14 54	− 19	− 10	14 25
5	Egmont Key,	27 36	82 46	5 31	11 26	16 57	− 23	− 20	16 14
6	Cedar Key,	28 58	82 57	5 32	13 9	18 41	− 26	− 45	17 30
7	St. Marks,	30 0	84 11	5 37	13 37	19 14	− 27	− 22	18 25
8	St. George's Island,	29 35	85 12	5 41	14 59	20 40	− 30	− 25	19 45
9	Pensacola,	30 18	87 15	5 49	10 53	16 42	− 22	− 17	16 3
10	Fort Morgan,	30 9	88 0	5 52	11 9	17 1	− 22	− 14	16 25
11	Cat Island,	30 6	88 38	5 55	12 53	18 48	− 26	− 81	17 1
12	South West Pass,	28 56	89 22	5 57	10 23	16 20	− 21	− 21	15 38
13	Dernière Island,	28 58	90 58	6 4	13 37	19 41	− 27	− 24	18 50
14	Calcasieu,	29 40	93 21	6 13	14 56	21 9	− 30	− 17	20 22
15	Bolivar Point,	29 23	94 46	6 19	16 47	23 6	− 34	− 12	22 20
16	Galveston,	29 18	94 41	6 19				− 30	
17	Aransas Pass,	28 15	96 31	6 28	14 30	20 58	− 29	− 17	19 12
18	Brazos Santiago,	26 6	97 10	6 29	14 45	21 14	− 29	− 15	20 30

The lagging of the diurnal wave behind the semi-diurnal which at the Tortugas was 4^h, 23^m, at the South West Pass is 4^h 49^m. The mean computed depth of the portion of the Gulf traversed by the wave from the semi-diurnal wave is 1666 fathoms and from the diurnal 666 fathoms, for the mean result of the two 1000 fathoms. The actual depth has not been ascertained, but probably does not exceed 1000 fathoms. From this line of deep water the semi-diurnal wave reaches the stations on the western coast of the Florida peninsula in their order from south to north and west. The movement west of St. George's appears to be in the order of Pensacola, Fort Morgan, and Cat Island, while for the diurnal wave it was Cat Island, Fort Morgan, and Pensacola. At South West Pass there is a sudden increase of establishment as if another semi-diurnal wave brought the tide there; the mean establishment of the six stations west of South West Pass is 20^h, 15^m, while that of the six east of it is 17^h, 31^m, a difference of about three hours. This table with the remarks already made in regard to the appearance of two high waters in the curves for Isle Dernière and Calcasieu, indicate a system of interferences yet to be unravelled. As was the case with the diurnal wave, the stations at Isle Dernière and Calcasieu furnish cotidal hours nearly like those of Brazos Santiago, and Aransas, and Galveston is later than either. Upon the whole then, there is a general resemblance in the motion of the two waves as assigned by observation with some considerable discrepancies. The annexed table No. 6 shows the difference between the cotidal hours of the diurnal and semi-diurnal waves at the several stations.

TABLE 6.

Comparison of Establishments of semi-diurnal and diurnal tides, in the Gulf of Mexico.

	Stations.	Diff. between diurnal & ½ diurnal	
		h.	m.
1	Cape Florida,	6	28
2	Indian Key,	8	16
3	Key West,	9	22
4	Tortugas,	9	26
5	Egmont Key,	7	54
6	Cedar Keys,	8	21
7	St. Marks,	8	39
8	St. George's Island,	5	19
9	Pensacola,	10	57
10	Fort Morgan,	10	12
11	Cat Island,	9	17
12	South West Pass,	10	4
13	Dernière Island,	7	19
14	Calcasieu,	5	53
15	Bolivar Point,	6	17
16	Aransas Pass,	5	59
17	Brazos Santiago,	4	48

When we come to follow these results into the discussion of the groups, they are far from satisfactory. Perhaps this was to have been expected from the circumstances before stated, the groups were nevertheless elaborately examined, though without much fruit.

The table of groups (Table 7) is arranged as for the diurnal tides, containing as before a number for reference, the names of the stations, and their latitude and longitude, the values of the coefficients of each, the angle made by the cotidal line with the meridian, the movement of the wave perpendicular to the cotidal line expressed by the number of minutes employed in traversing a mile, and the number of miles per hour.

TABLE 7.

	Stations.	Mean Longitude.	Mean Latitude.	Mean co-tidal hour.	M Diff. of cotidal hour for one geog. mile of Longit'de.	N Diff. of cotidal hour for one geog. mile of Latit'de.	Angle $\tan \frac{N}{M}$ Co tidal angle.	Diff. of cotidal hour correspd'ding to one geog mile perpend. to cotidal lines. $\sqrt{M^2+N^2}$	Miles per hour tidal wave
		° ′	° ′	h m.			° ′		
A	Cape Florida, Indian Key, Key West,	80 55	25 0	13 12	1·515	1·151	37 14	1·902	31·5
B	Indian Key, Key West, Tortugas,	81 52	24 38	13 47	-0·551	-1·284	-66 49	1·401	42·8
C	Egmont Key, Cedar Key, St. Mark's,	83 18	23 51	17 22	0·049	0·935	-87 0	0·935	64·2
D	Fort Morgan, Cat Island, South West Pass,	88 40	29 44	16 21	-1·260	1·887	56 16	2·269	26·4
E	Isle Dernière, Calcasieu, Bolivar Point,	93 2	29 20	20 37	-1·245	-1·517	50 36	1·963	30·6
F	South West Pass, Dernière Island, Calcasieu, Bolivar Point, Brazos,	93 7	28 37	19 32	-1·243	1·488	50 7	1·939	30·9

Groups A and B, composed of Cape Florida, Indian Key, and Key West, and of Indian Key, Key West and Tortugas, especially the first, give plausible results and the computed establishments vary but 1·5 mins. at the greatest, from the observed. I have not been able to form any satisfactory connection between these groups and those on the western coast of the peninsula.

The next group which gives a tolerable result is Egmont Key, Cedar Keys, and St. Marks. In this the direction of the cotidal line, the velocity and the establishments are satisfactory. The establishment of St. George's station is irregular and is very probably erroneous. The semi-diurnal wave is composed of two very small ones, and it has been necessary to reconcile the discrepancies which they presented, sometimes one being the governing tide and sometimes the other.

Group D, composed of Fort Morgan, Cat Island, and South West Pass is the next which gives a good result. E, composed of Isle Dernière, Calcasieu and Bolivar Point, and F, of all the stations from South West Pass to Brazos Santiago except Aransas, give good results as to direction and velocity.

The computed establishments as in the case of the diurnal wave present considerable discrepancies from the observed. The least difference is $8^m·5$ and the greatest 67^m. These groups are marked upon the chart No. 6 and the cotidal hour next before and after the mean cotidal hour of the groups.

An approximation to the cotidal lines from these data is also shown upon the chart. The corrected cotidal hours of the several stations are marked upon the chart.

In comparing the two sets of cotidal lines for the diurnal and semi-diurnal waves we find a general resemblance in the great bay between the western coast of Florida, and the eastern coast of Louisiana, the lines of 24, 25, and 26 of the diurnal tide on the eastern side of the bay corresponding generally with 16, 17, and 18 of the semi-diurnal tide, and 25 and 26 hours of the diurnal tide on the western side of the bay corresponding generally to 16 and 17 of the semi-diurnal. On the southern coast of Florida by the Keys on the contrary the lines of 19, 20, 21, 22, and 23 hours succeed each other rapidly between Cape Florida and the Tortugas in the diurnal series, while 13 and 14 hours occur only along the same shores in the semi-diurnal tide. On the contrary, in the bay between Louisiana and Texas, or west of South West Pass, the lines of 25, 26 and 27 hours occur only at considerable distances in the diurnal system, while 16, 17, 18, 19, 20, and 21 occur in the same space, between South West Pass and the Brazos Santiago in the semi-diurnal tide. I shall continue to collect observations bearing upon the facts discussed in this paper and to have them worked up, and so as to amend the imperfections of the approximate results now presented. There

are simultaneous observations at some of the stations which were formerly examined with but little satisfaction as to the conclusions; these will now be resumed and may throw additional light upon the results at some of the doubtful stations.

The interference problems will be taken up when more extended data give better hopes of a satisfactory solution of them.

ART. II.—*Notes on the Progress made in the Coast Survey, in Prediction Tables for the Tides of the United States Coast;* by A. D. BACHE, Superintendent U. S. Coast Survey.

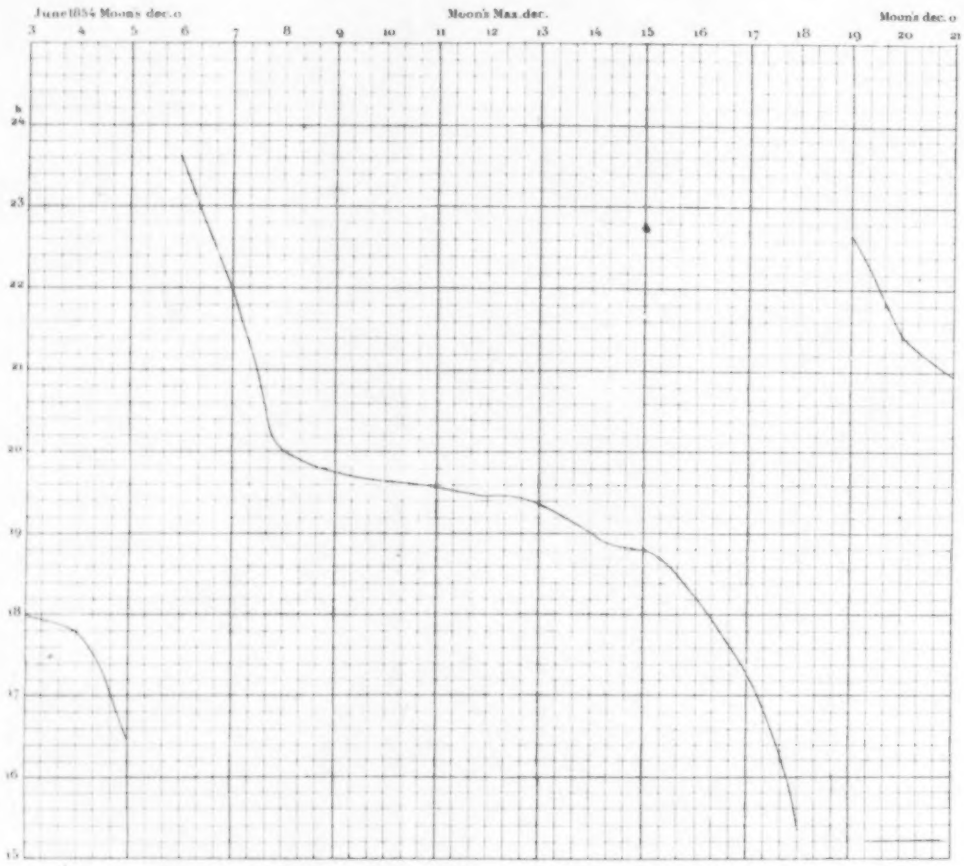
(Communicated to the American Association for the Advancement of Science, by authority of the Treasury Department.)

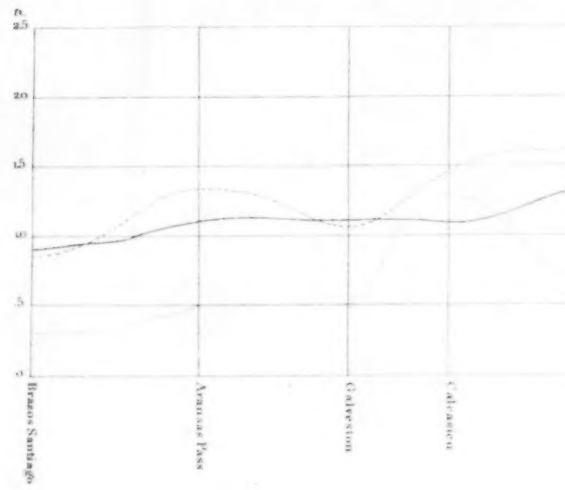
As soon as tidal observations had accumulated sufficiently to make the task a profitable one, I caused them to be treated, under my immediate direction, by the methods in most general acceptance. The observations of Old Point Comfort, Virginia, were among the earliest used for this purpose, and the labors of Commander Charles H. Davis, U. S. N., then an Assistant in the Coast Survey, were directed to their reduction, chiefly by the graphical methods pointed out by Mr. Whewell. This work was subsequently continued by Mr. Lubbock's method, by Mr. Henry Mitchell: and next the tides of Boston harbor were taken up as affording certain advantages in the observations themselves, which could not be claimed for those of Old Point.

The system of Mr. Lubbock is founded on the equilibrium theory, and in it the inequalities are sought by arranging the elements of the moon's and sun's motions, upon which they depend. Having obtained the co-efficient of the half monthly inequality of the semi-diurnal tide at Boston, from seven years observations, through the labors of the tidal division, and approximate corrections for the parallax and declination, I was much disappointed in attempting the verification by applying them to individual tides for a year during which we had observations. There was a general agreement on the average, but discrepancies in the single cases, which were quite unsatisfactory. Nor were these discrepancies without law, as representing their residuals by curves did not fail to show. By introducing corrections for declination and parallax of the moon increasing and decreasing, we reduced these discrepancies, but still the results were not sufficient approximations. With the numerical reductions of the observations before referred to, was commenced in 1853, under my immediate direction, by Mr. L. W. Meech, a study of the theory of the tides directed chiefly to the works of Bernouilli, La Place, Airy, Lubbock and Whewell. The immediate object

DIAGRAM II.

Showing the variations in the lunitidal intervals of diurnal high water according to Moon's declination at Egmont Key, Tampa Bay, Florida





U.S.COAST SURVEY

A.D.BACHE Supdt.

DIAGRAMS OF

HEIGHTS & LUNITIDAL INTERVALS OF DIURNAL T

in the

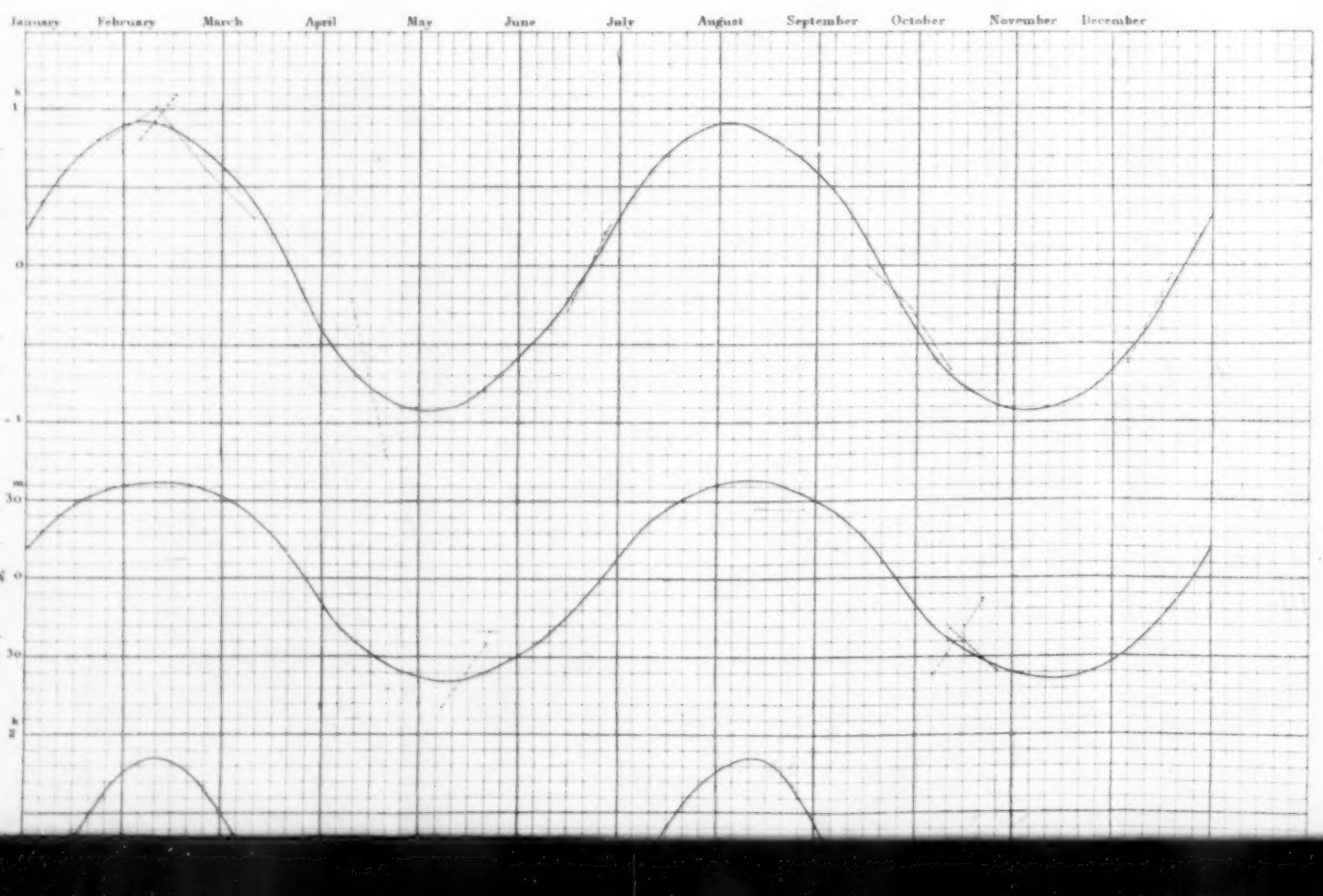
GULF OF MEXICO

From observations by the U.S.Coast Survey

1856

DIAGRAM IV.

Variations of lunitidal intervals of diurnal wave from mean of whole year.



Lith of J.Bien 491 Fulton St. N.Y.

DIAGRAM I.

Showing the height of the tides & the distances between the tidal stations in the Gulf of Mexico

Heights of { observed tides ——— / semidiurnal tides ——— / diurnal tides .....

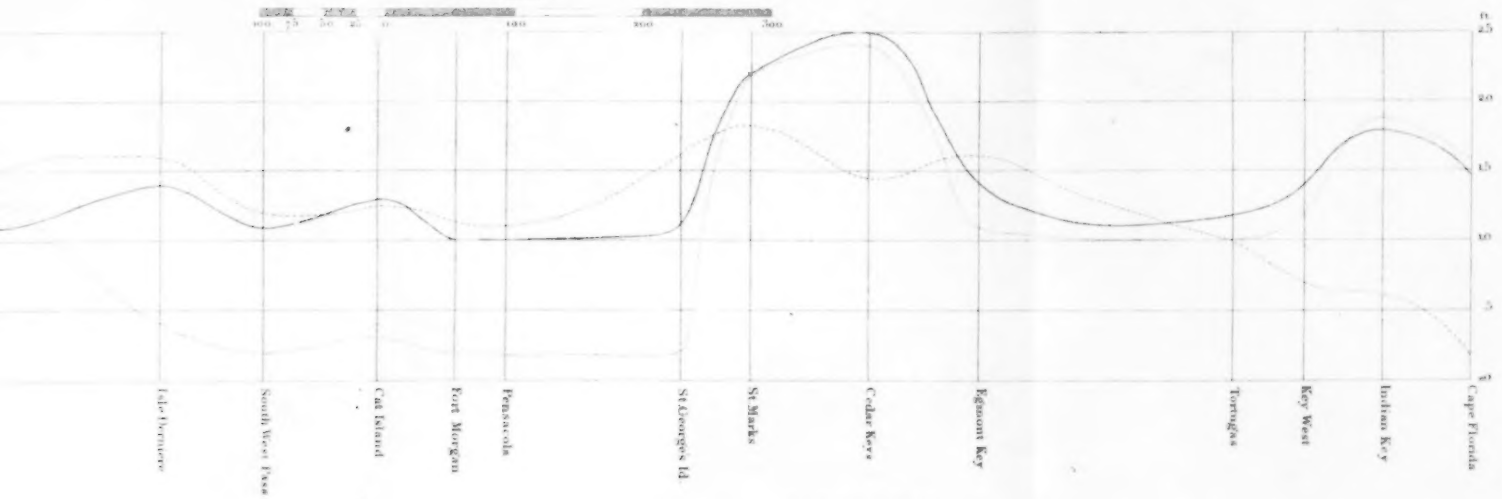
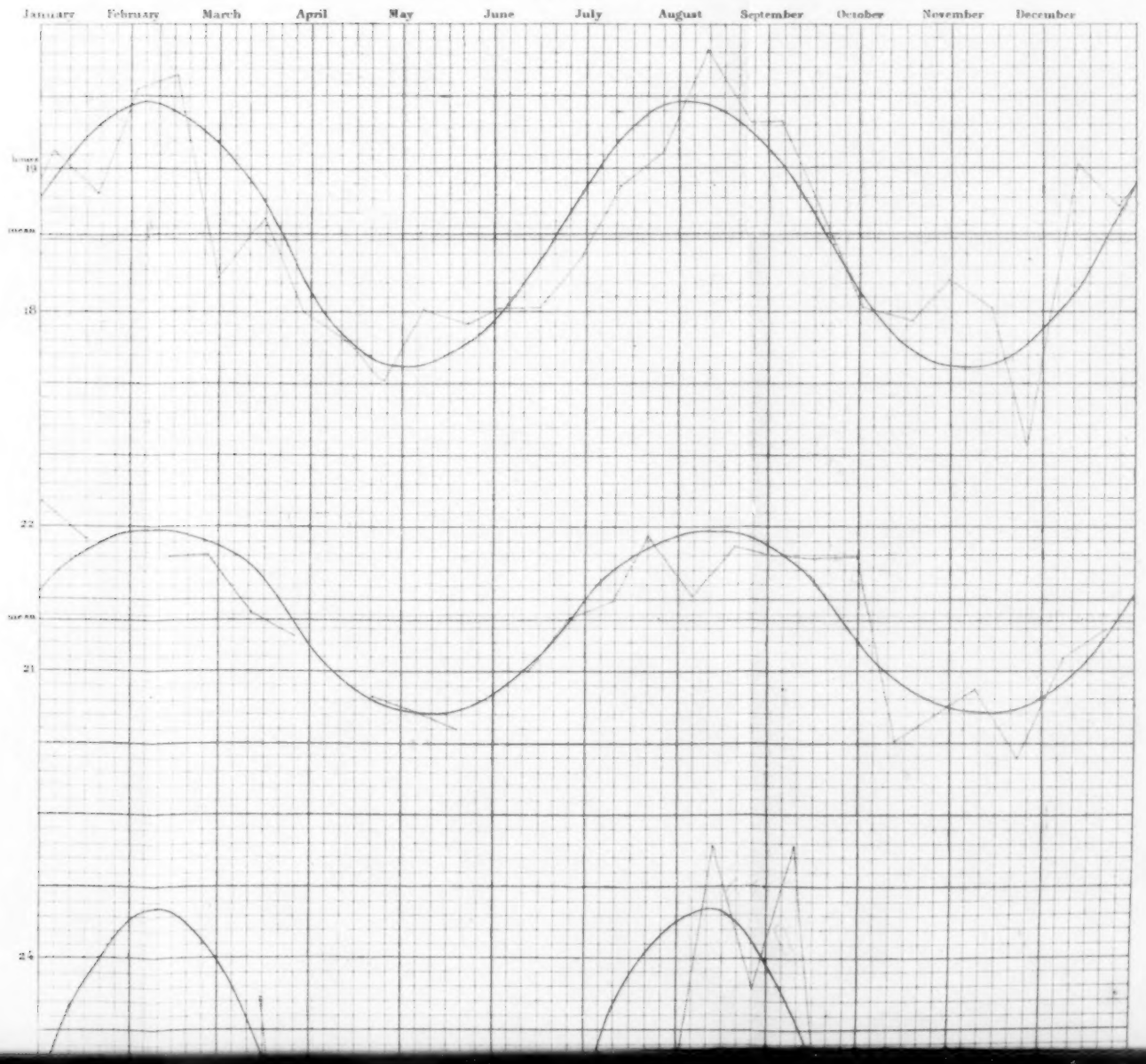
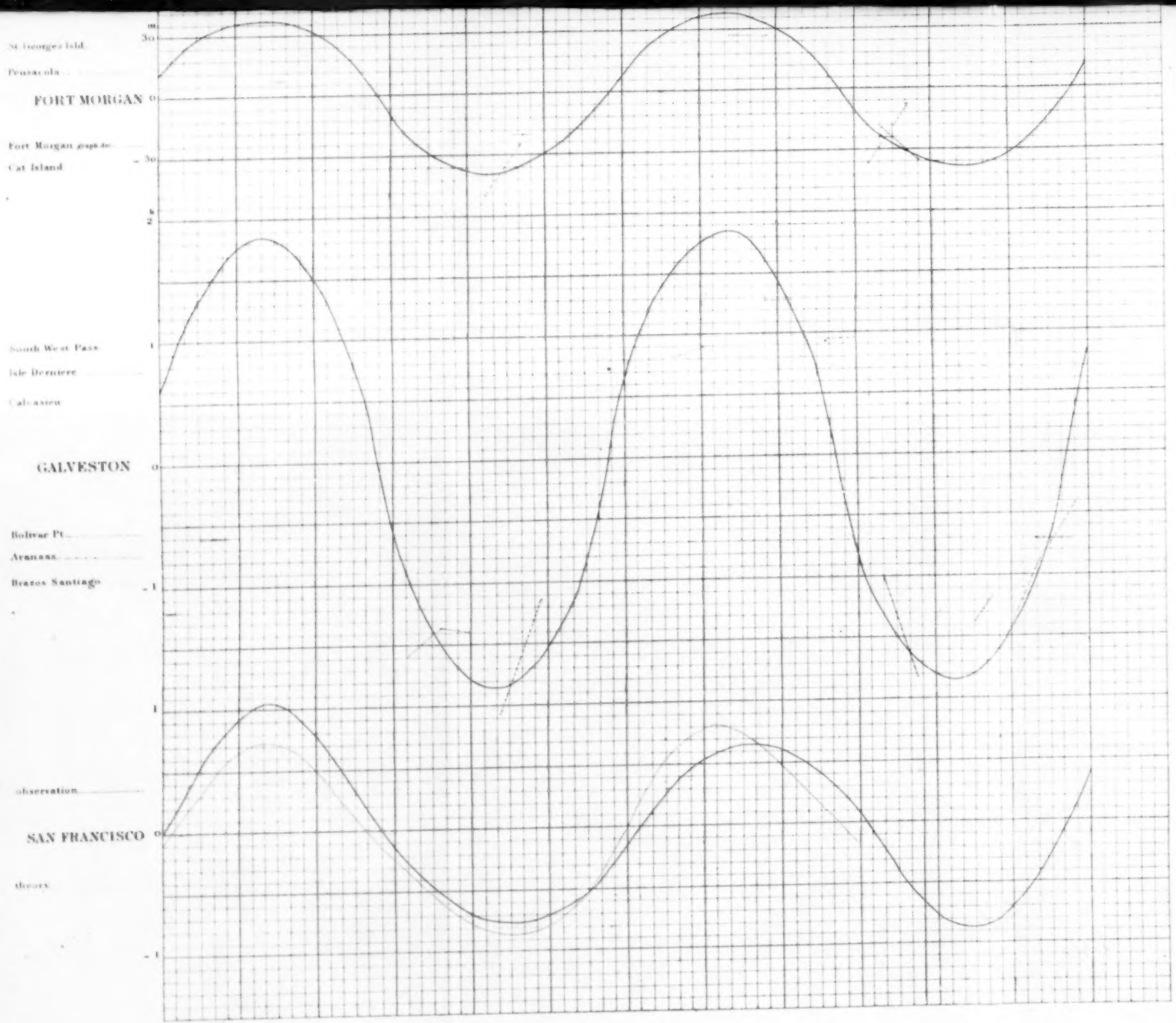


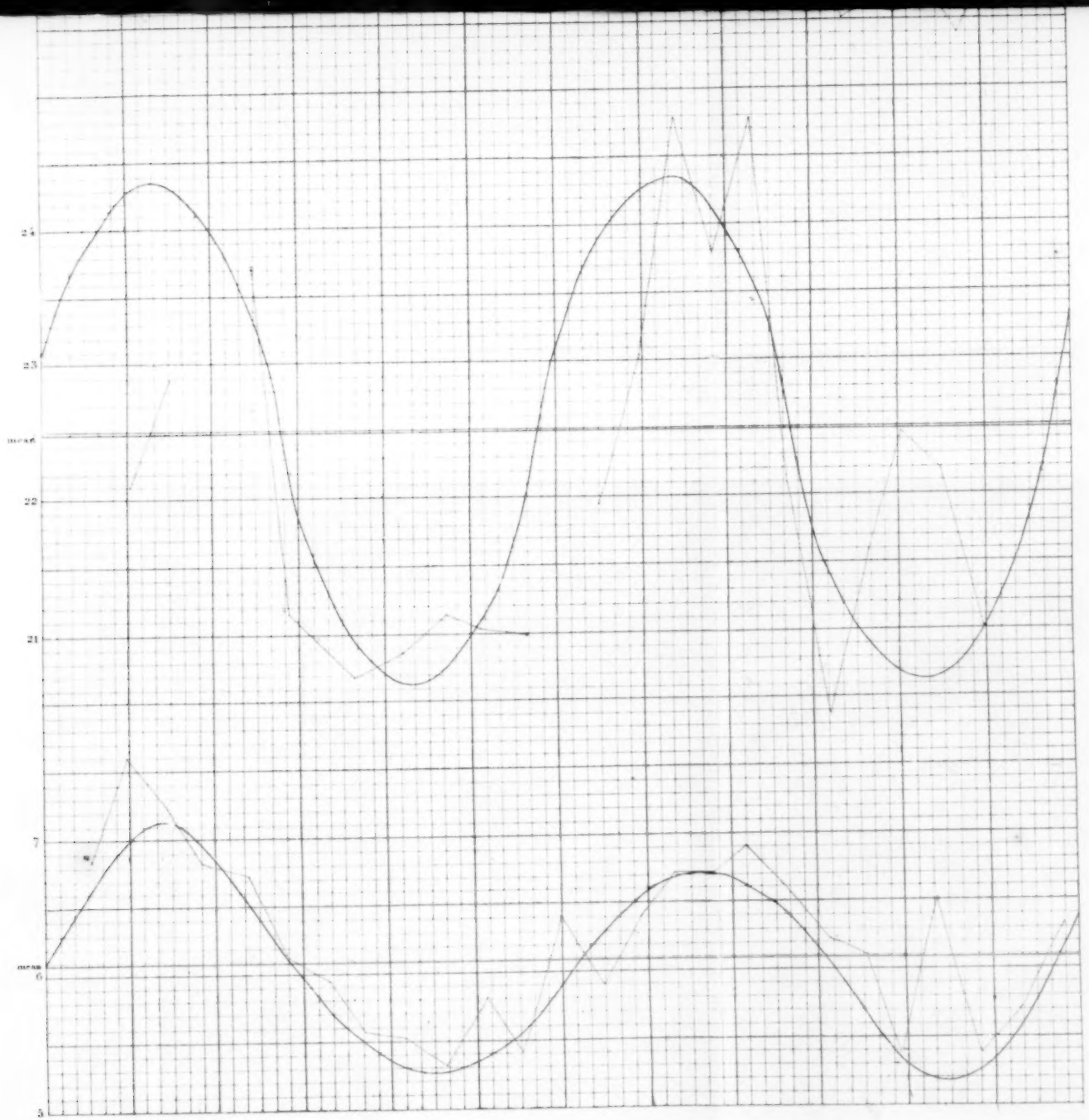
DIAGRAM III.

Representing the annual variations of the lunitidal intervals of diurnal high water

Observations ——— Graphical correction ———




St Georges Isld
Pensacola
FORT MORGAN
Fort Morgan
Cat Island
South West Pass
Isle Derniere
Galveston
GALVESTON
Bolivar Pt
Aransas
Brazos Santiago
observation
SAN FRANCISCO
theory
m
30
0
- 30
h
2
1
0
- 1
1
0
- 1


24
23
22
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6
5

which I had in view was the application of the wave theory to the discussion of our observations. I thought that the mind of an expert mathematician, directed entirely to the theoretical portions of this work, with directions by a physicist, and full opportunities of verifying results by extended series of observations, the computations of which should be made by others in any desired form, would give, probably, the best results in this combined physical and mathematical investigation.

The general form of the different functions expressing the tidal inequalities is the same in the different theories, and may be said on the average to be satisfactory as to the laws of change which these inequalities present. Whether we adopt, with La Place, the idea that periodical forces produce periodical effects, or with Airy that the tidal wave arrives by two or more canals: or with Bernouilli and Lubbock, the results of an equilibrium spheroid, or with Whewell, make a series of inequalities, semi-menstrual, parallax, and declination, with different epochs, we arrive at the same general results, that the heights and times of high water, may be represented by certain functions, with indeterminate co-efficients, in the form of which the theories in a general way agree. By forming equations from the observations and obtaining the numerical values of the co-efficients, by the method used so commonly in astronomical computations, the result is accomplished.

A general consideration of the co-ordinates in space of the moon and sun, without any special theory, would lead to the same results, representing the lunitidal intervals by series of sines and co-sines, with indeterminate co-efficients.

Calling I the luni-tidal interval from observation, λ the mean luni-tidal interval, H the clock time of observation, $l't$ the moon's longitude, P' the moon's parallax, $\delta P'$ the hourly variation of the moon's parallax, we have for the formula representing the correction for half monthly inequality, $s \sin 2H + s_1 \cos 2H$; for the moon's parallax correction, $p(P'-57') + p_2(P'-57') \sin 2H + p_3(P'-57') \cos 2H$; for the correction for hourly difference of the moon's parallax, $p_1(\delta P') + p_4(\delta P') \sin 2H + p_5(\delta P') \cos 2H$, and for the moon's declination corrections including the rate of change $d \sin 2l't + d_1 \cos 2l't + q_1 \sin 2l't \sin 2H + q_2 \sin 2l't \cos 2H + q_3 \cos 2l't \sin 2H + q_4 \cos 2l't \cos 2H$. There are corresponding terms for the inequalities produced by the sun's action. The whole formula takes the form:

$$I = \lambda + s \sin 2H + s_1 \cos 2H \quad \left\{ \text{Mean interval and half monthly inequality correction.} \right.$$

$$p(P'-57') + p_2(P'-57') \sin 2H + p_3(P'-57') \cos 2H \quad \left\{ \text{Moon's parallax correction.} \right.$$

$$p_1(\delta P') + p_4(\delta P') \sin 2H + p_5(\delta P') \cos 2H \quad \left\{ \text{Hourly difference of moon's parallax correction.} \right.$$

$$d \sin 2l't + q_1 \sin 2l't \sin 2H + q_2 \sin 2l't \cos 2H. \quad \left. \right\} \text{Moon's declination corrections.}$$

$$d_1 \cos 2l't + q_3 \cos 2l't \sin 2H + q_4 \cos 2l't \cos 2H. \quad \left. \right\}$$

$$+t_1 \sin lt \sin 2H + t_2 \sin lt \cos 2H$$
$$+t_3 \cos lt \sin 2H + t_4 \cos lt \cos 2H \quad \text{Sun's parallax corrections.}$$
$$+Q_1 \sin 2\,lt \sin 2H + Q_2 \sin 2\,lt \cos 2H$$
$$+Q_3 \cos 2\,lt \sin 2H + Q_4 \cos 2\,lt \cos 2H \quad \text{Sun's declination corrections.}$$

The grouping of the observations of one year at Boston, to apply this method, the formation of the equations and their solution by the method of indirect elimination has been the work of Mr. R. S. Avery, who has labored most assiduously and successfully, ingeniously checking his work where the system of checks could be applied, at every step. He has determined the values of λ and of the co-efficients for Boston, as follows:—

$\lambda = +38{\cdot}47$, $d = -3{\cdot}17$, $d_1 = -35{\cdot}62$, $p = -0{\cdot}93$, $p_1 = -1{\cdot}56$
$s = -19{\cdot}49$, $s = +11{\cdot}97$, $p_2 = +1{\cdot}31$, $p_3 = -1{\cdot}21$, $p_4 = +0{\cdot}23$
$p_5 = +0{\cdot}60$, $q_1 = -7{\cdot}17$, $q_2 = +1{\cdot}81$, $q_3 = +2{\cdot}91$, $q_4 = -1{\cdot}99$
$t_1 = +5{\cdot}14$, $t_2 = +2{\cdot}26$, $t_3 = -0{\cdot}76$, $t_4 = -1{\cdot}37$
$Q_1 = -21{\cdot}25$, $Q_2 = +28{\cdot}39$, $Q_3 = +27{\cdot}10$, $Q_4 = +23{\cdot}13$

There are propositions for facilitating this work, growing out of the experience acquired in the computations, but requiring more examination than they have yet received before pronouncing upon them. It is possible that by applying Lubbock's method of averages to some of the terms, approximate values may be found more readily than by the method we have employed. Two additional terms for the sun's declination, $D \sin 2\,lt$, and $D_1 \cos 2\,lt$, will be introduced.

I present to the Association the tables computed by Mr. Avery for applying this method to the prediction of the tides at Boston harbor.

In order to test the co-efficients, computations were made for different parts of the months of the year 1853, for which we have observations. Transit C was used as the transit of reference. The differences between the predicted and observed results are shown in the annexed table, the first column of which contains the dates, the second the computed, the third the observed, and the fourth the observed *less* the computed results.

From the table it appears that in twenty pairs of tides, the morning and afternoon results being grouped in the fifth column to get rid of the diurnal inequality, there are two differences of less than 2^m, thirteen of more than 2^m and less than 4^m, three of more than 4^m and less than 10^m, two of more than 10^m. The probable error of the prediction of a single pair of tides is $4^m{\cdot}12$.

These laborious researches are still in progress, but I have thought that the results already obtained, required a notice of them, and a recognition of the labors of Messrs. Meech and Avery.

Comparison of observed and predicted times of high water, Boston, Mass.

DATE, 1853,	Time of high water.				Diff. Obs Pr. M	Mean of Pairs M.
	Predicted.		Observed.			
	h.	m.	h.	m.		
March - - - 21	8	4·7	8	3	− 1·7	
" - - - 21	20	32·9	20	32	− 0·9	− 1·3
" - - - 25	11	28·0	11	21	− 7·0	
" - - - 25	23	49·8	23	48	− 1·8	− 4·4
" - - - 29	2	21·7	2	20	− 1·7	
" - - - 29	14	45·3	14	42	− 3·3	− 2·5
April - - - 2	6	16·9	6	21	4·1	
" - - - 2	18	51·5	18	59	7·5	5·8
" - - - 6	10	19·8	10	18	− 1·8	
" - - - 6	22	40·2	22	36	− 4·2	− 3·0
June - - - 21	11	18·4	11	18	− 0·4	
" - - - 21	23	44·7	23	49	4·3	2·0
" - - - 25	2	34·5	2	39	4·5	
" - - - 25	15	2·3	15	3	0·7	2·6
" - - - 29	5	57·7	6	7	9·3	
" - - - 29	18	24·3	18	37	12·7	11·0
July - - - 3	9	27·4	9	31	3·6	
" - - - 3	21	52·2	21	53	0·8	2·2
" - - - 7	0	0·1	0	3	2·9	
" - - - 7	12	10·3	12	12	1·7	2·3
September - - 24	3	59·4	4	7	7·6	
" - - - 24	16	24·8	16	24	− 0·8	3·4
" - - - 28	7	39·7	7	44	4·3	
" - - - 28	20	11·6	20	15	3·4	3·9
October - - - 2	11	6·1	11	4	− 2·1	
" - - - 2	23	31·1	23	30	− 1·1	− 1·6
" - - - 6	1	44·7	1	40	− 4·7	
" - - - 6	14	7·7	14	7	− 0·7	− 2·7
" - - - 10	5	24·5	5	19	− 5·5	
" - - - 10	17	57·8	17	58	0·2	− 2·7
December - - 21	3	7·2	3	9	1·8	
" - - - 21	15	28·4	15	30	1·6	1·7
" - - - 25	6	32·6	6	31	− 1·6	
" - - - 25	19	0·7	18	52	− 8·7	− 5·2
" - - - 29	10	22·4	10	26	3·6	
" - - - 29	22	53·3	22	42	− 11·3	− 3·9
January - - - 2	1	29·4	1	2	− 27·4	
" - - - 2	13	54·0	13	41	− 13·0	− 20·2
" - - - 6	4	45·8	4	53	7·2	
" - - - 6	17	33·9	17	30	− 3·9	1·7

Final mean, - - - - - - - - - - 0·5

Probable error, minutes, - - - - 4·12

Number of differences, less than 2 minutes - - - - = 2

Number of differences more than 2 minutes, and less than 4 - = 13

" " " 4 " - - 10 - = 3

" " " 10 " - - - - = 2

ART. III.—*Observations to determine the cause of the increase of Sandy Hook, made by the Coast Survey for the Commissioners on Harbor encroachments of New York;* by Prof. A. D. BACHE, Sup. U. S. Coast Survey.—(Abstract.)

IT is known as one of the developments of the Coast Survey that the peninsula of Sandy Hook is gradually increasing, growing to the northward into the main ship channel. A spot north of the Hook where there was forty feet of water when Captain Gedney made his survey, in less than ten years was nearly bare at low water. The importance of determining the cause of this increase, as leading to the means of controlling it, cannot be overestimated. The Commissioners on Harbor Encroachments had early attended to this matter, and requested that the necessary observations for its investigation should be made. These were made under my immediate direction, by Henry Mitchell, one of the sub-assistants in the Coast Survey, with all desirable zeal and ability. Various causes had been assigned for this growth, by the action of the waves and winds sometimes on the outer side and sometimes on the inside of the Hook. The effect of the opening and closing of Shrewsbury Inlet had also been insisted upon. To examine these and other probable causes, laborious observations of tides and currents had been made in the vicinity at stations marked upon the map presented to the Association. Careful measurements of the low water line had also been made in connection with these observations, and with others, of the force and direction of the wind. Objects easily distinguished from the land, and of various specific gravities and shapes, had been deposited near the shore of the Hook to determine the power and direction of transportation of matter along the shores of the Hook. It is easy to see how laborious all of these observations are, and that some of them are obtained with considerable danger: hence the credit to be given to Mr. Mitchell may be measured. The results of the observations have not yet been worked out in all their detail, but the conclusions from them are perfectly safe, and are of the highest importance. It turns out that this growth of the Hook is not an accidental phenomena, but goes on regularly, and according to determinable laws. The amount of increase depends upon variable causes, but the general fact is, that it increases year by year; and the cause of this is a remarkable northwardly current, the amount and duration of which these observations assign along both shores of the Hook, the outer one extending across the whole breadth of False Hook channel with varying velocity, and the one inside of the Hook extending nearly one-third of the distance across Sandy Hook bay. These currents run to the north during both the ebb and flood tide, with varying rates, and result from these tides

directly and indirectly. The inner current is the one by which the flood and ebb tides draw, by the lateral communication of motion, the water from Sandy Hook bay, and the outer is similarly related to those tides as they pass False Hook channel. The velocities and directions which have been found, prove this conclusively. An important observation for navigation results from this, for more than seven hours out of the twelve there is a northwardly current running through False Hook channel, which assists vessels entering New York harbor on the ebb tide, and is to be avoided in passing out with the ebb. This northwardly current runs on the inside for eleven hours out of the twelve. It is the conflict of these two northwardly currents outside and inside, and the deposit of the materials which they carry to the point of the Hook, which causes its growth. Within a century, it has increased a mile and a quarter, and at about the rate of one-sixteenth of a mile a year, on the average, for the last twelve years. Flynn's knoll, on the north side of the main ship channel, does not give way as the point of the Hook advances. The importance of watching this movement cannot, therefore, be overstated. The mode of controlling the growth is obvious from the results obtained. The observations are still continued to obtain the necessary numerical results.

ART. IV.—*Notice of Observations to determine the Progress of the tidal wave of the Hudson River, made by the Coast Survey for the Commissioners on Harbor Encroachments;* by A. D. BACHE, Supt. U. S. Coast Survey.—(Abstract.)

PROF. BACHE explained the importance of a knowledge of the movement of the tide wave up the Hudson River to the determination of the shore line of New York bay and harbor, and the subject of encroachment upon the area of the harbor, and stated that the New York Commissioners had directed a full series of observations to be made for the examination of this point in the bearing just referred to, and also on the character of the improvements projected for the Hudson river at the Overslaugh, and indeed in the whole distance from Troy to New Baltimore. Nine tidal stations were in the course of occupation between Governor's Island, New York, and Greenbush, opposite Albany. At the two terminal stations Saxton's self-registering guages were placed, and he had expected to invite members to see the Albany guage, but the late freshet in the river had required its removal for the present. Prof. Bache explained the selections made of the localities of the tidal stations and the reasons for their special positions, and stated that the results of the work would hereafter be laid before the Association.

ART. V.—*On the Characteristic Action of the Barometer during the passage of a Revolving Storm, such as a Hurricane or Tornado, being a small rise and not a great fall;* by JOHN CHAPPELL-SMITH.

I WISH to call attention to some facts which afford evidence that a great fall in the barometer is not an essential element in all storms, as Redfield and Espy have assumed in their storm theories; that, on the contrary, the effect on the barometer of hurricanes and tornadoes, or of all storms accompanied with electric explosions, is a rise when the axis or centre of the storm passes near it. Some of the facts which lead to this conclusion will be found in the Smithsonian Contributions to Knowledge, in an article by me on a tornado near New Harmony, Indiana, April 30, 1852. In that article, the bearing of these facts on the subject was not pointed out, because I was not then impressed with their full force; my object was chiefly to point out facts directly opposed to the rotary or cyclonic theory of storms, so far as the proof of that theory depends on the direction in which bodies are prostrated by the progress of a storm. After a careful examination of all that had appeared in this journal in support of Mr. Redfield's views by himself and others, I could not resist the conclusion that the method of collecting and presenting the facts by the parties who had investigated storm phenomena was inconclusive; to pass over the field of the wreck of a tornado, select a tree here and a tree there, place them on a diagram, then assert that these were sufficient proofs of the rotary or cyclonic theory of storms is not enough. A much more laborious examination than this appeared to me to be necessary to attain just views. A tract of a square mile, at least, required to be plotted, with the magnetic bearings and relative distances of the prostrated bodies. This labor I performed, and gave, in the contribution referred to, a plot of a square mile, on which is shown the magnetic bearing and relative distances of nearly 7000 trees which had been prostrated by the tornado in little more than a minute. Sections made in a like manner across the track, at 2 and at 8 miles distance, were also given, which show a uniformity in the mode of action of the prostrating power; the whole furnishing, as I think, a complete refutation of Mr. Redfield's theory, so far as he has attempted to establish it by evidences of this kind.

But my object in this paper is not to dilate on what has been done in that contribution, but to point out the bearing of the barometrical facts, contained therein, upon Mr. Redfield's theory; facts which prove that the low barometer, which frequently exists at the time of the passage of a tornado or hurricane, is not caused by the tornado, but is dependent on some other cause.

The facts to which I principally refer are set forth in the following diagram. Here it will be perceived that from the morning

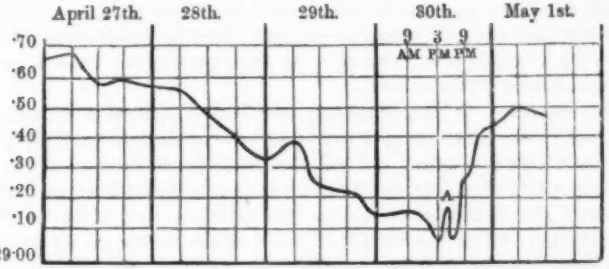


Diagram representing the barometric fluctuations on the day of the tornado, and on the three preceeding days at New Harmony, Indiana.

The horizontal lines are equal to fluctuations amounting to $\frac{1}{10}$ of an inch. The vertical lines represent the height of the barometer at the time of sunrise; 9 A. M. 3 P. M., and 9 P. M. on each day; the darker lines representing the sunrise observations.

A represents the fluctuation of the barometer between the hours of 3 and 6 P. M., on the 30th of April, at which hours the atmosphere was calm and clear; but between which the tornado came up, committed its ravages and passed on.

of April 27th, to 3 P. M., on the 30th, the barometer had been slowly and gradually falling, the only interruptions appearing to be those due to the maximum horary oscillation at 9 A. M., the wind at any time during the period scarcely exceeding a gentle breeze; at 3 P. M., a little more than an hour previous to the centre of the storm's passing the meridian of New Harmony, all was calm, and, with the exception of a slight haze, clear, without any sign to indicate the near approach of so fearful a storm. Soon after this, sounds of distant thunder were heard, and at 4 P. M., there was slight rain, strong wind, vivid lightning and loud thunder; the storm now began to exert its terrific violence, the rain fell in torrents accompanied by a shower of hailstones of extraordinary size, the wind blew in all directions and the barometer had *risen* ·050 of an inch. By 4·30 P. M., the storm had reached its height, and the barometer had *risen* ·030 of an inch more. The storm now began to abate, the barometer to *fall*, and by 5·30 P. M., all was again calm and clear, the barometer standing at the same point at which it stood previous to the coming up of the storm. Now this *rise* of near $\frac{1}{10}$ of an inch in the barometer, which commenced as the storm began to rage, which reached its maximum when the storm reached its height, and this *fall* which began as the storm began to abate, which reached its minimum as the storm cleared off, is surely a very significant fact; and if, as my experience, aided by that of others, has shown, this be a constant characteristic of the barometer in all storms accompanied with electric explosions; if the barometer invariably rises as the centre of a storm passes near an observer, though that rise seldom exceeds $\frac{1}{10}$ of an inch, does it not

give strong grounds for concluding that the condition of low barometer, which often exists at the time of the passage of a whirlwind or tornado, is not caused by these meteors; does not the low barometer appear to be due to some other cause, and to be only a contingent, and not, as storm theorists have assumed, a conditional phenomenon?

In reference to the diagram, at the first glance a person may regard it as presenting a remarkable confirmation of Mr. Redfield's views: a great fall of the barometer as the axial area of the whirlwind approaches, and then a great rise as it passes away. But a little consideration will show that it is widely at variance with his views. Let us refer to them as given in his last communication to this Journal in September, 1854. He there states that the normal condition of the lower wind stratum consists of a constant occurrence and progression of cyclones from the equator in various degrees of activity; he defines a cyclone to be a moving disk or stratum of rotating atmosphere, which sometimes manifests itself by light and feeble, and sometimes by strong and violent, winds; the more inert and passive cyclones, he says, constantly occupy in their transit the greater portion of the earth's surface, and move in orbits corresponding to the more active cyclones traced on his storm charts; and that the effect on the barometer of any cyclone is proportionate to the general activity of the rotation considered as a whole. In another place he describes the effect as depressing the barometer, and says that the intensity of the depression rapidly increases as the axial area of the whirlwind approaches; that this axial area is the point of greatest depression, and that the latter is obviously due to the centrifugal force of the revolving motion in the body of the storm.

Now bearing these views of Mr. Redfield in mind, let us consider them in connection with the fall of the barometer represented in the diagram above. The fall, which commenced at New Harmony on the 27th of April, three days previous to the axial area of the whirlwind's or cyclone's arrival there, cannot be supposed to be due to the advancing portion of the rotating disk of the cyclone which reached New Harmony on the 30th, because, the distance of the axial area at the time must have been upwards of 4000 miles, supposing that it moved uniformly with the velocity with which it was known to move over 300 miles of the track, viz., 60 miles per hour; but suppose that its average velocity was 30 miles per hour, this would make the distance of the axial area upwards of 2000 miles, a distance so great, that Mr. Redfield would allow, that it would be preposterous to suppose that the fall of the barometer on the morning of the 27th could be caused by the advance of a storm whose axial area at that time was at that distance.

I will now give some facts which appear to prove that the tornado of the 30th did not exercise any influence at New Harmony up to one and a half hours of its axial area reaching there. From a slight account of the barometric condition which existed over the United Sates at about the time of the occurrence of the tornado at New Harmony, for which account I acknowledge my obligations to Professor Henry of the Smithsonian Institution, it appears that in all the States in the basin of the Ohio, including Wisconsin, Iowa and Missouri, a very low barometer prevailed; while a high barometer prevailed in the New England States, Pennsylvania and Virginia; and a very high one in Carolina, Florida, Georgia, Alabama, Louisiana and Mississippi. In the valley of the Ohio, on an area of the diameter of 400 miles, of which New Harmony is the centre, the barometer at 3 P. M. stood at nearly 29·100 inches. At this time the atmosphere at New Harmony was perfectly calm, although at about the distance of 90 miles, a storm of about 30 miles diameter was making its way through this area of low barometer with a velocity of 60 miles per hour; a storm which had the power over a track one mile in width to prostrate trees, to twist and bend them, as if they were reeds, at the rate of 7000 a minute, although many of them had a circumference of several feet. As this storm or cyclone approached, the barometer was watched, but there was not any indication of that direct barometric depression which Mr. Redfield says is produced by the increasing rapidity of the leftwise rotation of the cyclone in approaching its axial area. Instead of the intensity of the barometric depression rapidly increasing as the axial area of the whirlwind approached, which Mr. Redfield says is so obviously due to the centrifugal force of the revolving motion in the body of the storm, the barometer was seen actually to *rise* as the axial area approached, and to *fall* as it passed away. What becomes then of the theories built upon this assumed barometric depression? a depression which Redfield and Espy aver must be [as a general thing, EDS.] a consequence of such meteors, and on which assumed enormous depression they have based their theories. On a future occasion, in addition to a statement of further barometrical observations of my own, I will strengthen my position by a reference to the experience of celebrated navigators and meteorologists who advance facts similar to those which I have recited, but who, apparently influenced by a popular belief, have failed to notice their significant bearing on the question.

The phenomena of the New Harmony tornado which I have referred to, also offers insuperable objections to the cyclonic theory of storms when considered in connection with Mr. Redfield's idea of their origin. He says that the cyclones originate in the tropics, and are mainly the result of the mechanical gravitation of the atmosphere as connected with the rotative and orbital

movements of the earth's surface; he says that their earliest activity and violence may be explained by the action of local currents; that opposite winds may coalesce in a vast gyration, instead of following their usual stratiform course without interference with each other: and that when once the fall of the barometer and the involute vortical movement has been established, the extraneous and tangential forces of contiguous winds is not necessary to continue the action; for the law of centrifugal action must produce an accumulation of pressure beyond the active verge of the whirlwind, and the pressure of the external atmosphere alone, around the basin of the storm, constantly keeps up the involute vertical movement, and is sufficient to maintain the existing vortical action.

Now I ask those who entertain this idea, to view it in connection with the conditions that existed in the valley of the Ohio on the afternoon of April 30th, particularly in the vicinity of New Harmony. Granting that the circumstances described by Mr. Redfield did generate a cyclone or rotary storm, is the cause he has assigned, consisting of the mere mechanical force of rotation, sufficient to impel a rotating disk of atmosphere of some 30 miles in diameter, as in the tornado in question, many hundreds of miles, which disk had carried with it, and still maintained, as it entered the valley of the Ohio, a power that could prostrate thousands of trees on successive miles in successive minutes, which swept off houses, and carried cattle up far in the air?* To me the thing is inconceivable, the cause is not adequate to the effects; and it is the more inconceivable when it is known that, in this, as in other storms of the kind, the destructive action was *intermittent*, that some parts of the track were passed over without destruction, and that in others it was exerted with various degrees of violence. This spasmodic action is not in accordance with the inherent mechanical force ascribed to the cyclone; and some other cause is requisite for the production of these effects of this now retarded, and now accelerated, mechanical force. But Mr. Redfield says that it is not requisite; extraneous aid, he says, is not necessary to keep up the vortical action, for when once the fall of the barometer and the involute vortical motion has been established, the law of centrifugal action must produce an accumulation of pressure beyond the active verge of the whirlwind, and this pressure alone, around the basin of the storm, must keep up the involute vortical action. But, as experience shows in the New Harmony tornado, in opposition to Mr. Redfield's theory, there was not any diminution of pressure near

* The writer has here mistaken the views of Mr. Redfield, who makes the movement of the cyclone over the earth's surface in no sense a consequence of the rotation: and who has further shown that spasmodic action is a necessary consequence when the axis of the cyclone is not vertical.—Eds.

the axial area of the storm, and there was not any accumulation beyond its active verge, as I have shown in describing the condition of the surrounding area of 400 miles in diameter through which the tornado progressed.

Of course the same objections apply to Mr. Espy's theory: he admits that a tornado may very greatly depress the barometer; and "if it should depress it more than 3 inches" then, he says, that something more is necessary than is provided for in his theory,—"perhaps electricity." It is not a little remarkable that when Mr. Espy presented his storm views to the British Association, Sir John Herschel observed, that if an ascensional column be the cause of storms, the barometer *ought to rise in the centre of the storm or column;* a fact which the observation of what actually occurs during the passage of a tornado has now verified; for whether the storm progress in the form of an ascensional column, or of a cyclone, the facts I have advanced prove that the *barometer does rise in its centre.*

ART. VI.—*On the Spirality of Motion in Whirlwinds and Tornadoes;* by W. C. REDFIELD.

Read before the American Association at Albany, Aug. 26, 1856.

1. AN aggregated spiral movement, around a smaller axial space, constitutes the essential portion of whirlwinds and tornadoes.

2. The course of the spiral rotation, whether to the right or left, is one and the same in this respect throughout the entire whirling body, so long as its integrity is preserved. But the oblique inclination which the spiral movement also has to the plane of the horizon, is in opposite directions as regards the interior and exterior portions of the revolving mass. Thus, in the outward portion of the whirlwind the tendency of this movement is obliquely downwards, when the axis is vertical; but in the interior portion, the inclination or tendency of the spiral movement is upward. This fact explains the ascensive effects which are observed in tornadoes and in more diminutive whirlwinds.

3. Owing to the increased pressure of the circumjacent air in approaching the earth's surface, the normal course of the gradually descending movement, in a symmetric whirlwind, is that of an involuted or closing spiral; while the course of the interior *ascending* movement of rotation is that of an evolved or opening spiral. Hence, the horizontal areas of the higher portions of the whirl exceed greatly those of its lower portions.

4. The area of the ascending spiral movement in the vortex, as it leaves the earth's surface, is by far the smallest portion of

the whirling body; for the reason that the rotation here is proportionally more active and intense, being impelled by the aggregated pressure and momentum of the more outward portion of the whirlwind as it converges from its larger area, on all sides, by increasingly rapid motion, into the smaller area of ascending rotation.* That this interior portion of the whirl resembles an inverted hollow cone, or column, with quiescent and more rarified air at its absolute center, may be inferred from the observations which have been made in the axial portions of the great cyclones. Into this axial area of the tornado the bodies forced upward by the vortex cannot fall, but will be discharged outward, from the ascending whirl. The columnar profile of this axial area sometimes becomes visible, as in the water spouts so called.

5. Accessions caused by circumjacent contact and pressure are constantly accruing to the whirling body, so long as its rotative energy is maintained. A correlative diffusion from its ascending portion must necessarily take place, towards its upper horizon; and this is often manifested by the great extent or accumulation of cloud which results in this manner from the action of the tornado. In other words, there is a constant discharge from the whirling body in the direction of least resistance.

6. The spirality of the rotation and its inclination to the horizon, in the great portion of the whirl which is exterior to its ascending area, is not ordinarily subject to direct observation. Nor is the outline or body of the more outward portion of the whirlwind at all visible, otherwise than in its effects.

7. In *aqueous* vortices the axial spiralities of the exterior and interior portions are in reverse direction to those in the atmosphere, the descending spiral being nearest to the axis of the vortex. Hence, lighter bodies and even bubbles of air are often forced downward in the water, in the manner in which heavier bodies are forced upwards in the atmosphere.

The foregoing is simply a statement of results which I have derived from a long course of observation and inquiry. It does not include the partial and imperfect exhibitions of whirlwind action, which often occur; nor the various movements and phenomena which are collaterally associated with tornadoes and whirlwinds, some of which are of much significance.

* The law of increment in the velocity of the whirlwind, as it gradually converges into smaller areas by its spiral involution, is that which pertains to other bodies when revolving around interior foci towards which they are being gradually drawn or pressed nearer and nearer, in their involute course;—the line of focal or centripetal pressure, thus sweeping *equal areas in equal times*, at whatever diminution of distance from the center; except as the velocity may be effected in degree by the resistance of other bodies. Such resistance is of little effect in a tornado, because its revolving mass is mainly above all ordinary obstacles, such as orchards and forests, into which the spirally *descending* and accelerated blast, near the contracted extremity of the inverted and truncated cone of the whirl, penetrates with constant freshness and intensity of force; already acquired in the higher and unobstructed region.

ART. VII.—*On the Application of the Mechanical Theory of Heat to the Steam Engine*; by R. CLAUSIUS.

[Concluded from vol. xxii, p. 374.]

39. I believe that it will not be without interest, if before I attempt to make these equations more convenient for application, I show how we may also, for an imperfect steam engine, arrive at the same expressions by the inverse method formerly pointed out as by that previously followed. In order, however, not to be too prolix in this digression, I will take into consideration only two of the imperfections which are considered in the foregoing equations, namely, the presence of the injurious space, and the less pressure of the steam in the cylinder than in the boiler during the influx. On the other hand I will assume that the expansion is complete, in which case we must put $T_3 = T_0$ and that also the quantities T'_0, T''_0 and T'''_0 are equal to each other.

We have to apply in this determination the equation (2), which we will here write in the following form:

$$W' = \frac{1}{A}\left(Q_1 - T_0 \int_0^{Q_1} \frac{dQ}{T}\right) - \frac{T_0}{A} N.$$

The first term on the right side signifies the work which we should obtain by means of the applied quantity of heat Q_1, which for our case is represented by $m_1 r_1 + Mc(T_1 - T_0)$, if these imperfections did not take place. This term is already calculated in § 23 where the following expression was found:

$$\frac{1}{A}\left[m_1 r_1 + Mc(T_1 - T_0) - T_0\left(\frac{m_1 r_1}{T_1} + Mc \log \frac{T_1}{T_0}\right)\right].$$

The second term signifies the loss of work which is occasioned by these two imperfections. The quantity N which occurs in it, is also already calculated, namely, in § 36, and is represented by the expression cited in equation (38).

If we substitute these two expressions in the foregoing equation, we have

$$(44)\quad W' = \frac{1}{A}\left[m_1 r_1 - \frac{T_0}{T_1} m_2 r_2 + Mc(T_1 - T_0) - (M+\mu)cT_0 \log \frac{T_2}{T_0} + \mu_0 r_0\right]$$

We easily see that this equation corresponds in fact with equations (XIV) if we introduce into the first of them the mass m_3 for the mass m_2, which may be done by means of the third equation, and by then putting $T_3 = T_0 = T'_0 = T'''_0$.

In the same manner we can take into account the loss of work which arises from the incompletе expansion, by calculating the uncompensated transformation which occurs during the passage

of the steam from the cylinder into the condenser and include this in N. By this calculation, which I will not here actually execute, we arrive completely at the expression given in (XIV) for the work.

40. In order now to be able to apply the equations (XIV) to a numerical calculation, it is first necessary to determine more nearly the quantities p'_1, p'_0 and p''_0.

No generally valid law can be established as to the manner in which the pressure changes in the cylinder during the influx, because the opening and closing of the steam pipe takes place in different machines in different ways.

Consequently it is impossible to assign, once for all, a definite value holding good for the relation between the mean pressure p'_1 and the final pressure p_2 when the last is strictly considered.

This however becomes possible by slightly changing the signification of p_2. The cutting off of the cylinder from the boiler cannot of course take place in an instant, but the necessary motion of the clapper or slide valve requires, according to the different feed arrangements, a greater or less time, during which the steam in the cylinder expands somewhat, because during the contraction of the opening less new steam can enter than corresponds to the velocity of the piston. We may therefore in general assume that at the end of this time the pressure is somewhat less than the mean pressure denoted by p_1.

If, however, we do not restrict ourselves to bring into calculation precisely the end of the time necessary for closing, as the moment of the cut-off, but allow some liberty in fixing this moment, we may obtain other values for p_2. We may then consider the point of time as so chosen, that if up to that time the whole mass M had flowed in, a pressure would take place at this instant which would be exactly equal to the mean pressure calculated up to this instant. If we put the instantaneous closure, more nearly determined in this manner, in the place of the gradual closure which actually takes place, we commit only an insignificant error with respect to the work calculated from it. We may therefore accept this modification of Pambour's assumption that $p'_1 = p_2$, whereby, however, it remains reserved for especial consideration for each particular case, to determine correctly the period of the cut-off, with reference to the prevailing circumstances of the case.

41. With respect to the counter pressure p'_0 which takes place during the return of the piston, the difference $p'_0 - p_0$ is evidently smaller under otherwise equal circumstances, the smaller p_0 is. It will therefore be smaller in machines with condensers than in machines without condensers in which p_0 is equal to one atmosphere. In the most important machines without condensers, the locomotives, a particular circumstance usually occurs

which contributes to increase the difference, namely: that we do not offer to the steam the shortest and widest possible channel for its passage into the atmosphere, but conduct it into the chimney, and there let it flow out through a somewhat narrow tube in order to produce in this way an artificial draught.

In this case an accurate determination of the difference is important for the reliability of the result. We must also consider that the difference is not constant in one and the same machine, but depends upon the rate of motion, and must determine the law according to which this dependence takes place. I will not however here enter upon these considerations and the investigations which have already been instituted upon this subject, since they have nothing to do with the mechanical theory of heat.

In machines in which this application of the steam which passes from the cylinder does not occur, and particularly in machines with condensers, p'_0 is so little different from p_0, and can therefore change so little with the rate of motion, that it is sufficient for most investigations to assume for p'_0 a mean value.

As further, the quantity p_0 occurs in equations (XIV) only in a term affected by the factor σ, and therefore exerts a very slight influence on the value of the work, we may without fear put for p_0 the value which is most probable for p'_0.

The pressure which takes place in the injurious space p''_0 depends as already mentioned upon whether the cutting off from the condenser takes place before or after the end of the motion of the piston, and may therefore vary greatly. But this pressure also, and the quantities which depend upon it, occur in equations (XIV) only in such terms as are affected with small factors namely, with μ and μ_0 so that we may satisfy ourselves with an approximate estimate and omit an accurate determination of this pressure. In such cases, where no particular circumstances lead us to believe that p''_0 deviates much from p'_0, we may neglect this difference, as well as that between p_0 and p'_0, and assume the value which represents the mean counter pressure in the cylinder with the greatest probability as a common value for all these quantities. This value may then simply be denoted by p_0; by the introduction of these simplifications, equations (XIV) pass into

$$(\text{XV}).\quad \begin{cases} W' = \frac{1}{A}\left[m_1 r_1 - m_3 r_3 + Mc(T_1 - T_3) + \mu_0 r_0 - \mu c(T_3 - T_0)\right] \\ \qquad\qquad + m_3 u_3 (p_3 - p_0) \\ m_2 r_2 = m_1 r_1 + Mc(T_1 - T_2) + \mu_0 r_0 - \mu c(T_2 - T_0) + A\mu_0 u_0 (p_2 - p_0) \\ \qquad\qquad + AM\sigma(p_1 - p_2). \\ \frac{m_3 r_3}{T_3} = \frac{m_2 r_2}{T_2} + (M + \mu) c \log \frac{T_2}{T_3}. \end{cases}$$

42. It is assumed in these equations that beside the masses M, m_1, μ and m_0, of which the first two must be known by direct observation, and the last two can be determined approximately from the magnitude of the injurious space, the four pressures p_1, p_2, p_3, p_0, or what is the same, the four temperatures T_1, T_2, T_3, T_0, are given. This condition is however only partially fulfilled for the cases which occur in practice, and we must therefore take other data to assist us in the calculation.

Only two of these four pressure-forces are to be supposed as known, namely p_1 and p_0, the first of which is given immediately by the boiler pressure-guage, and the last may be inferred, at least approximately, from the indications of the condenser-guage. The two others, p_2 and p_3, are not given, but instead of them we know the dimensions of the cylinder, and at what position of the piston the cut-off from the boiler takes place. From this we may deduce the volumes which the steam in the cylinder takes up at the moment of the cut-off and at the end of the expansion, and these two volumes may therefore take the place, as data, of the pressure-forces p_2 and p_3.

We have now to bring the equations into such a form that we can execute the calculation by means of these data.

43. Let us again denote, as in setting forth Pambour's theory, the whole space which becomes free in the cylinder during one stroke, including the injurious space, by v', the space which becomes free up to the cutting-off from the boiler, by $c\,v'$, and the injurious space by $\varepsilon\,v'$. Then we have, according to what has already been said, the equations

$$\begin{aligned} m_2 u_2 + (M+\mu)\sigma &= \varepsilon v' \\ m_3 u_3 + (M+\mu)\sigma &= v' \\ m_0 u_0 + \mu\sigma &= \varepsilon v'. \end{aligned}$$

The quantities μ and σ are both so small that we may neglect their product, so that we have

$$(45)\quad \begin{cases} m_2 u_2 = \varepsilon v' - M\sigma \\ m_3 u_3 = v' - M\sigma \\ \mu_0 = \dfrac{\varepsilon v'}{u_0}. \end{cases}$$

Furthermore, according to equation (VI) we have

$$r = A\,T u\,g,$$

if we introduce the letter g for the differential co-efficient $\frac{dp}{dT}$ contained in it, which will in what follows occur so often that a simpler notation is advantageous. According to this, we may replace the quantities r_2 and r_3 in the above systems of equations by u_2 and u_3. Then the masses m_2 and m_3 occur only in the products $m_2 u_2$ and $m_3 u_3$, and for these we may substitute the values given in the first two of equations (45).

In like manner, by means of the last of these equations, we may first eliminate the mass μ_0, and as for the other mass, this may it is true be somewhat greater than μ_0. As however the terms which contain μ as a factor are, in any event, very insignificant, we may substitute without hesitation also for μ the same value which is found for μ_0 that is, for the purpose of numerical calculation, we may drop the assumption made for the sake of generality, that the mass which was originally in the injurious space was partly fluid and partly vapor, and consider the mass in question as wholly in the form of vapor.

The substitution just signified may be effected in the general equations (XIV) as well as in the simplified equations (XV). As the substitution however presents no difficulties, we will here confine ourselves to the last, in order to obtain the equations at once in a form which is suited to numerical computation.

After this change they read as follows:

$$(\text{XVI})\begin{cases} W'=\dfrac{m_1 r_1+\dfrac{Mc(T_1-T_3)}{A}}{}-(v'-M\sigma)(T_3 g_3-p_3+p_0)+\varepsilon v'\dfrac{r_0-c(T_3-T_0)}{Au_0} \\ (ev'-M\sigma)T_2 g_2=\dfrac{m_1 r_1+\dfrac{Mc(T_1-T_2)}{A}}{}+\varepsilon v'\left(\dfrac{r_0-c(T_2-T_0)}{Au_0}+p_2-p_0\right) \\ \qquad\qquad +M\sigma(p_1-p_2) \\ (v'-M\sigma)g_3=(ev'-M\sigma)g_2+\left(M+\dfrac{\varepsilon v'}{u_0}\right)\dfrac{c}{A}\log\dfrac{T_2}{T_3}. \end{cases}$$

44. In order to refer these equations, which determine the work of one stroke or of the quantity of steam M_1, finally to the unit of weight of steam, the same process is to be applied by means of which formerly equations (35) were converted into (XII). We divide namely the three equations by m_1 and then put

$$\frac{M}{m_1}=l,\ \frac{v'}{m_1}=V,\ \frac{W'}{m_1}=W.$$

The equations now become

$$(\text{XVII})\begin{cases} W=\dfrac{r_1+\dfrac{lc(T_1-T_3)}{A}}{}-(V-l\sigma)(T_3 g_3-p_3+p_0)+\varepsilon V\dfrac{r_0-c(T_3-T_0)}{Au_0} \\ (eV-l\sigma)T_2 g_2=\dfrac{r_1+\dfrac{lc(T_1-T_2)}{A}}{}+\varepsilon V\left(\dfrac{r_0-c(T_2-T_0)}{Au_0}+p_2-p_0\right) \\ \qquad\qquad +l\sigma(p_1-p_2) \\ (V-l\sigma)g_3=(eV-l\sigma)g_2+\left(l+\dfrac{eV}{u_0}\right)\dfrac{c}{A}\log\dfrac{T_2}{T_3}. \end{cases}$$

45. The application of these equations to the calculation of the work may be made in the following manner. We determine the volume V, which belongs to the unit of weight of steam, by means of the evaporating power supposed to be known, and of the rate of motion which the machine thereby assumes. With the help of this value, we calculate in the first place from the

second equation the temperature T_2, then from the third the temperature T_3, and these we finally apply in the first equation to the determination of the work.

In this process we meet, however, a peculiar difficulty. In order to calculate the temperatures T_2 and T_3 from the two last equations, these ought properly to be solved according to the temperatures. They contain these temperatures however, not only explicitly, but also implicitly, inasmuch as p and g are functions of the temperature. The equations for further treatment would become too complicated, if we were to substitute for the elimination of these quantities one of the usual empirical formulas which express the pressure of the steam for p, and its differential co-efficient for g. We might now perhaps help ourselves out in the same manner as Pambour has done, namely, by setting up new empirical formulas which should be more convenient for the present purpose, and if not for all temperatures, at least within certain intervals, sufficiently accurate. I will not however, here make such attempts, but instead of them will direct attention to another process in which the calculation, it is true, is somewhat tedious, but easily executed in its single parts.

46. When the tension-series of the vapor is known with sufficient accuracy for any one fluid, we may calculate from it the values of the quantities g and Tg, for various temperatures, and unite them in tables just as it is customary to do with the values of p. I have executed such a calculation with the help of Regnault's tension-series for the vapor of water, which, up to this time, has been almost exclusively employed in steam engines, and for the interval of temperature within which the application holds good, namely, from 40° to 200° C.

I ought properly for this purpose to have differentiated the formulas which Regnault has used to calculate the single values of p below and above 100° according to t, and by means of the new formulas thus obtained to have calculated g. As however these formulas do not answer their purpose so completely as to make this tedious labor worth while, and as the establishment and calculation of another more appropriate formula would have been still more prolix, I have contented myself with using the numbers calculated for the pressure, for an approximate determination of the differential co-efficient of the pressure. If for instance, we denote the pressure for the temperatures 146° and 148° by p_{146} and p_{148}, I have assumed that the quantity

$$\frac{p_{148} - p_{146}}{2}$$

represents with sufficient accuracy the value of the differential co-efficient for the mean temperature 147.

For this purpose I have used above 100° the numbers cited by Regnault* himself. Moritz† has recently directed attention to the fact, with reference to the values under 100°, that the formula which Regnault has applied between 0° and 100° is somewhat inaccurate, particularly in the neighborhood of 100°, in consequence of the use of seven-figure logarithms in calculating the constants. Moritz has therefore calculated these constants with ten-figure logarithms, assuming the same values from observation, and has communicated the values of p deduced from this improved formula in so far as they differ from Regnault's, which first occurs above 40°. I have used these values.

After the quantity g is calculated for the single degrees of temperature, the calculation of the product Tg presents no longer any difficulty, since T is determined by the simple equation

$$T = 273 + t.$$

I have collected the values of g and $T\,g$, thus found in a table communicated at the end of this memoir. For the sake of completeness I have added the values of p belonging to them, those of Regnault above 100°, those calculated by Moritz below 100°. In each of these three series of numbers, the differences of every two successive numbers are given, so that we may find from this table, for every given temperature, the values of those three quantities, and conversely, for every given value of one of those three quantities, the corresponding temperature.

After what has already been said as to the calculation of g, I scarcely need to add that I do not consider the numbers of this table as accurate, but only communicate them for lack of better. As, however, the calculations which occur in the steam engine always rest upon rather uncertain data, we may apply for this purpose these numbers without hesitation, without having to fear that the uncertainty of the result will be thereby sensibly increased.

One observation is however still necessary. In equations (XVII) it is supposed that the pressure p and its differential co-efficient q are expressed in kilograms upon a square meter; in the tables, on the contrary, the same unit of pressure is retained to which Regnault's tension-series refers, namely, millimeters of mercury. In order, notwithstanding, to be able to apply the table, we only need in these equations to divide all the terms which contain neither p nor g as a factor by the number 13·596.

I will, for the sake of brevity, denote this number, which is nothing else than the density of mercury at 0° compared with that of water at its maximum density, by k.

This modification of the formulas produces almost no increase of calculation, inasmuch as it consists in substituting in every

* Mém. de l'Acad. des Sciences, T. xxi, p. 625.

† Bulletin de la Classe physico-mathematique de l'Acad. de St. Petersburg, T. xii, p. 41.

place, instead of the constant factor, which has according to Joule the value already cited, 423·55, the other constant.

$$(46) \qquad \frac{1}{Ak} = \frac{423{\cdot}55}{13{\cdot}596} = 31{\cdot}1525,$$

and besides, in place of the work W, the quantity $\frac{W}{k}$ is first found, which must then be multiplied by k.

47. Let us now return to equations (XVII) and first consider the second of them.

This equation may be written in the following form:

$$(47) \qquad T_2 g_2 = C + a(t_1 - t_2) - b(p_1 - p_2),$$

in which the quantities C, a, and b are independent of t_2, namely,

$$(47_a) \quad \begin{cases} C = \frac{1}{e\,V - l\sigma}\left[\frac{r_1}{Ak} + \varepsilon V\left(\frac{r_0 - c(T_1 - T_0)}{Aku_0} + p_1 - p_0\right)\right] \\ a = \frac{c\left(l + \frac{\varepsilon V}{u_0}\right)}{Ak(e\,V - l\sigma)} \\ b = \frac{\varepsilon V - l\sigma}{e\,V - l\sigma}. \end{cases}$$

Of the three terms on the right side of (47) the first preponderates by far, and hence it becomes possible to determine the product $T_2\ g_2$, and thereby at the same time the temperature t_2 by successive approximation.

In order to obtain the first approximate value of the product which we may call $T'g'$, substitute on the right side t_1 in the place of t_2, and in like manner p_1 in place of p_2, then we have

$$(48) \qquad T'\,g' = C.$$

The temperature t' belonging to this value of the product is to be looked for in the table. In order to obtain the second approximate value of the product, put the value t' just found and the corresponding value p' of the pressure on the right side of (47), for p_2 and t_2, whereby we obtain, taking the previous equation into consideration,

$$(48_a) \qquad T''\,g'' = T'\,g' + a(t_1 - t') - b(p_1 - p').$$

The temperature belonging to this value of the product t'' may be determined as before from the table. If this do not represent the sought temperature t_2 with sufficient accuracy, repeat the same process. Substitute on the right side of (47), t'' and p'' in place of t_2 and p_2, by which we obtain, taking the previous equations into consideration,

$$(48_b) \qquad T'''\,g''' = T''\,g'' + a(t' - t'') - b(p_1 - p''),$$

and can find the new value of the temperature t''' in the table.

In this manner we might continue as long as we please; but the third approximate value differs only by the $\frac{1}{1000}$ of a degree, and the fourth by less than the $\frac{1}{1000000}$ of a degree, from the true value of the temperature t_2.

48. The treatment of the third of the equations XVII is quite similar. If we divide this by $V-l\sigma$, and for the sake of more easy calculation introduce Briggs's logarithms, which may be denoted by the symbol Log., in place of the natural logarithms denoted by the symbol log., in which case it is only necessary to add the modulus M of this system as a divisor, the equation takes the form

$$g_3 = C + a \log \frac{T_2}{T_3}, \tag{49}$$

in which C and a have the following values independent of T_3.

$$\begin{cases} C = \dfrac{e\,V - l\sigma}{V - l\sigma} \cdot g_2 \\ a = \dfrac{c\left(l + \dfrac{\varepsilon\, V}{u_0}\right)}{M \,.\, Ak\,(V - l\sigma)}. \end{cases} \tag{49a}$$

In equation (49) the first term on the right side is again preponderant, so that we may apply the process of successive approximation. If we substitute in the first place T_2 in the place of T_3, we obtain as a first approximate value of g_3:

$$g' = C, \tag{50}$$

and can find in the table, the temperature t' which belongs to it, and from this easily form the absolute temperature T'. If we substitute this in (49) for T_3, we have

$$g'' = g' + a \log \frac{T_2}{T'}, \tag{50a}$$

from which T'' is found. In like manner, we obtain farther

$$g''' = g'' + a \log \frac{T'}{T''}. \tag{50b}$$

49. It only remains to determine the quantities c and r in order to be able to proceed to the numerical application of equations XVII. The quantity c, that is the specific heat of the liquid, has been considered as constant in the foregoing development. This, it is true, is not quite correct, since the specific heat increases somewhat with an increase of temperature. The errors cannot however be significant, if we select as a common value the value which is correct at about the middle of the interval which embraces the temperature occurring in the investigation. In engines driven by steam, 100° may be taken as such a mean temperature, which in a common high pressure engine with a con-

denser is about equally distant from the temperature of the boiler and that of the condenser. We will accordingly apply to water, the value which according to Regnault represents the specific heat at 100° putting

$$c = 1{\cdot}0130. \tag{51}$$

To determine the quantity r, we set out from the equation which Regnault has established for the whole quantity of heat which is necessary in order to warm a unit of weight of water from 0° to the temperature t, and to convert it into steam at this temperature, namely,

$$\lambda = 606{\cdot}5 + 0{\cdot}305\,.\,t.$$

If we substitute in this for λ, the sum corresponding to the previous definition $\int_0^t c\,dt + r$, we have

$$r = 606{\cdot}5 + 0{\cdot}305\,.\,t - \int_0^t c\,dt.$$

We must apply in the interval for c, the temperature function more accurately determined by Regnault, in order to obtain exactly the values of r which Regnault gives. I believe however that it is sufficient for our present purpose to employ in this case also for c the constant before cited. Hence we obtain

$$\int_0^t c\,dt = 1{\cdot}013\,.\,t,$$

and may now contract into one the two terms of the preceding equation depending on t, which reads $-0{\cdot}708\,.\,t$.

We must, at the same time, also change somewhat the constant term of the equation, and we will so determine it that that observation-value of r which is probably most accurate of all is also correctly represented by the formula. Regnault has found for the quantity λ at 100°, as a mean of 38 observations, the value 636·67. If we subtract from this the quantity of heat which is requisite to heat the unit of weight of water from 0° to 100°, and which according to Regnault is equal to 100·5 units of heat, there remains, if we satisfy ourselves with one decimal,

$$r_{100} = 536{\cdot}2.*$$

By employing this value, we obtain for r the formula

$$r = 607 - 0{\cdot}708\,.\,t. \tag{52}$$

A comparison of some of the values calculated from this with those given by Regnault† in his table will show that this simpli-

* Regnault himself does not give in his table exactly the above number but 536·5. This arises however only from the fact that instead of the previously mentioned value 636·67 he has put for λ at 100° in the calculation in round numbers 637.

† Mém. de l'Acad. des Sciences, T. xxi, p. 748.

fied formula corresponds with sufficient accuracy to the more strict mode of calculation before signified.

t	0	50°	100°	150°	200°
r acc. to (52),	607·0	571·6	536·2	500·8	465·4
r acc. to Regnault,	606·5	571·6	536·5	500·7	464·3

50. In order to be able to distinguish in their action the two different kinds of expansion to which the two last of equations XVII refer, it appears to me advantageous to consider, in the first place, a steam engine such that only one of them occurs in it. We will therefore begin with a machine which works without expansion.

In this case we must put for the quantity e, which signifies the ratio of the volumes before and after expansion, the value 1, and at the same time make $T_3 = T_2$, whereby equations XVII assume a simpler form.

The last of these equations becomes identical and therefore disappears. Furthermore, many terms of the first equation which only differ from the corresponding terms of the second in this, that the first contain T_3 and the others T_2, now become equal to them, and may therefore be eliminated. Hence we obtain by introducing at the same time, the above-mentioned quantity k,

$$\text{(XVIII)} \quad \begin{cases} \dfrac{W}{k} = V(1-\varepsilon)(p_2 - p_0) - l\sigma(p_1 - p_0) \\ (V - l\sigma)\, T_2 g_2 = \dfrac{r_1 + lc(T_1 - T_2)}{Ak} \\ \qquad + \varepsilon V\left(\dfrac{r_0 - c(T_2 - T_0)}{Aku_0} + p_2 - p_0\right) + l\sigma(p_1 - p_2). \end{cases}$$

The first of these two equations is exactly the same as that which we obtain according to Pambour's theory, if we put in (XII) $e=1$ and introduce the volume V in place of the quantity B. The difference exists thus only in the second equation, which has taken the place of the simple relation between volume and pressure assumed by Pambour.

51. Let us assume the quantity ε occurring in these equations, which represents the injurious space as a fraction of the whole space which becomes free for the steam, as equal to 0·05. The quantity of liquid which the steam carries with it, on its entrance into the cylinder, is different in different machines. Pambour says that it amounts on the average in locomotives to 0·25, in stationary steam engines however, to much less, perhaps to 0·05 of the whole mass which enters the cylinder. We will use the last datum for our example, according to which the ratio of the whole mass which enters the cylinder to the portion of it which is in the form of steam is as 1 to 0·95. Furthermore, let the pressure in the boiler be assumed to be 5 atmospheres,

to which the temperature 152°·22 belongs, and suppose that the machine has no condenser, or what is the same a condenser with a pressure of 1 atmosphere. The mean counter pressure in the cylinder is then greater than 1 atmosphere. In locomotives, this difference, as above mentioned, may by a particular circumstance become considerable, in stationary steam engines on the contrary, it is less. Pambour, in his numerical calculations for stationary machines without condensers, has quite neglected this difference, and as our object here is only to give an example for the comparison of the new formulas with those of Pambour, we will in this also agree with him, and put $p_0=1$ atmosphere. The following values accordingly come into application in equations (XVIII) for this example:

$$(53)\qquad \begin{cases} \varepsilon = 0{\cdot}05 \\ l = \dfrac{1}{0{\cdot}95} = 1{\cdot}053 \\ p_1 = 3800 \\ p_0 = 760. \end{cases}$$

If we assume in addition the once for all fixed values

$$k = 13{\cdot}596$$
$$\sigma = 0{\cdot}001,$$

there remain in the first of the equations (XVIII), besides the sought quantity W only the quantities V and p_2.

52. We must now first find out what is the least possible value of V.

This value corresponds to the case in which the same pressure takes place in the cylinder as in the boiler; and we need only substitute p_1 in the place of p_2, in the last of equations (XVIII). Hence we have

$$(54)\qquad V = \frac{\dfrac{r_1}{Ak} + l\sigma \,.\, T_1 g_1}{T_1 g_1 - \varepsilon\left(\dfrac{r_0 - c(T_1 - T_0)}{Aku_0} + p_1 - p_0\right)}.$$

In order to give an example at once of the influence of the injurious space, I have calculated two values from this expression, that which would arise if no injurious space were present, and therefore $\varepsilon = 0$, and that which must ensue from the supposition made by us that $\varepsilon = 0{\cdot}05$. These two values are expressed as fractions of a cubic meter for one kilogram of steam passing out of the boiler,

0·3637 and 0·3690.

That the last of these two values is greater than the first, arises from the fact that, in the first place, the steam penetrates into the injurious space with great velocity, the living force of this motion is then converted into heat, and this again causes the evaporation of a portion of the liquid mechanically carried along; and that,

in the second place, the steam which was in the injurious space before the influx, in like manner, contributes to increase the whole quantity of steam which is afterward present.

If we substitute the two values found for V in the first of equations (XVIII) whereby ε is at one time made $= 0$ and the other time $= 0{\cdot}05$, we obtain as corresponding quantities of work expressed in kilogram-meters,

14990 and 14450.

According to Pambour's theory, it makes no difference, with reference to the volume, whether one portion of it is injurious space or not; it is determined in both cases by the same equation (29_b) if we substitute in it for p the particular value p_1. Thereby we obtain

0·3883.

That this value is greater than that previously found for the same quantity of steam, 0·3637, may be explained from this, that we have hitherto considered the volume of steam at the maximum density, as greater than it can be according to the mechanical theory of heat, and this earlier view finds also its expression in equation (29_b.)

If we determine by means of this volume the work under the two suppositions that $\varepsilon = 0$ or $= 0{\cdot}05$, we have

16000 and 15200.

These quantities of work are, as was to be foreseen as immediate consequences of the greater volume, both greater than those beforehand, but not in an equal ratio, inasmuch as the loss of work occasioned by the injurious space is less, according to the equations developed by us, than it should be according to Pambour's theory.

53. In a machine of the kind considered here, which Pambour studied in action, the velocity which the machine actually assumed, is to that which may be calculated from his theory, as a minimum velocity for the same power of evaporation and the same pressure in the boiler, in one experiment, as 1·275 : 1, and in another with a less load, as 1·70 : 1. The volumes 0·495 and 0·660 would correspond for our case to these velocities. We will now choose as an example for the determination of the work, a velocity which lies between these two, by putting in round numbers $V = 0{\cdot}6$.

We have now first to find the temperature t_2 for this value of V. For this purpose equation (47) which assumes the following special form will serve:

$$(55) \qquad T_2 g_2 = 26577 + 56{\cdot}42 \,.\, (t_1 - t_2) - 0{\cdot}0483 \,.\, (p_1 - p_2).$$

If we execute by means of this equation, the successive determination of t_2 described in § 47, we obtain in succession the following approximate values:

$$t' = 133°{\cdot}01$$
$$t'' = 134\ {\cdot}43$$
$$t''' = 134\ {\cdot}32$$
$$t'''' = 133\ {\cdot}33.$$

Still further approximations would differ only in the higher decimal places, and we have accordingly, inasmuch as we will content ourselves with two decimals, to consider the last number as the true value of t_2. The pressure belonging to this is

$$p_2 = 2308{\cdot}30.$$

If we apply these values of V and p_2, at the same time with the other values closely determined in § 51, to the first of equations (XVIII) we obtain

$$W = 11960.$$

Pambour's equation (XII) gives for the same volume 0·6, the work

$$W = 12520.$$

54. In order to show still more distinctly the dependence of the work upon the volume, and at the same time, the difference which prevails in this respect between Pambour's and my theory, I have executed the same calculation as for the volume 0·6 for a series of volumes increasing at equal distances. The results are comprised in the following table. The first horizontal series of numbers, which is separated from the others by a line, contains the values found for a machine without injurious space. For the rest the arrangement of the table is easily understood.

V	t_2	W	According to Pambour.	
			V	W
0·3637	152°·22	14990	0·3883	16000
0·3690	152 ·22	14450	0·3883	15200
0·4	149 ·12	14100	0·4	15050
0·5	140 ·83	13020	0·5	13780
0·6	134 ·33	11960	0·6	12520
0·7	129 ·03	10910	0·7	11250
0·8	124 ·55	9880	0·8	9980
0·9	120 ·72	8860	0·9	8710
1	117 ·36	7840	1	7440

We see that the quantities of work calculated according to Pambour's theory, diminish more rapidly with the increasing volume than those calculated according to our equations, so that while they are at first perceptibly larger than the latter, they gradually approach nearer to them, and at last actually become smaller. This is explained from the fact that according to Pambour's theory, in the expansion which takes place during the influx, the same mass always remains in the form of steam, which was so in the beginning; according to our theory, on the contrary, a part of the mass which is carried along in the fluid

state, subsequently evaporates, and the more so the greater the expansion.

55. We will now in a similar manner consider a machine which works *expansively*, and we will for this purpose choose a machine with a condenser. With reference to the amount of expansion, we will assume that the cut-off from the boiler takes place when the piston has passed through one-third of its stroke. We have then to determine e the equation

$$e-\varepsilon=\tfrac{1}{3}(1-\varepsilon),$$

and hence we find, if we retain for ε the value 0·05,

$$e=\frac{1\cdot1}{3}=0\cdot3666\ldots.$$

Let the pressure in the boiler be, as before, assumed at five atmospheres. The pressure in the condenser with good arrangements can be maintained under one-tenth of an atmosphere. As it is not however always so small, and as, besides, the counter pressure in the cylinder somewhat exceeds the pressure in the condenser, we will assume for the mean counter pressure p_0, in round numbers, one-fifth of an atmosphere, or 152^{mm}, to which the temperature $t_0=60°\cdot46$ belongs. If we retain finally for l the value previously assumed, the quantities which come into application in this example are the following:

$$(56)\quad \begin{cases} e=0\cdot36667. \\ \varepsilon=0\cdot05. \\ l=1\cdot053. \\ p_1=3800. \\ p_0=152. \end{cases}$$

It only remains now, in order to be able to calculate the work to give the value of V. In order to have a fixed point in choosing it, we must first know the least possible value of V. This is found, exactly as in the case of machines without expansion, by putting p_1 in the second of equations (XVII) in the place of p_2, and changing in like manner the other quantities connected with p. We find in this manner for our case the value

$$1\cdot010.$$

Setting out from this we will assume as a first example that the actual velocity of the machine exceeds the least possible velocity in about the ratio of 3 : 2, putting in round numbers

$$V=1\cdot5,$$

and we will determine the work for this velocity.

56. In the first place, the two temperatures, t_2 and t_3, must be determined by substituting this value of V in the last two of equations (XVII). The determination of t_2 has already been somewhat more closely considered in the case of the machine without condenser, and as the present case is distinguished from that one only in this, that the quantity e, which was there put

$=1$, has here another value, I will not enter upon the subject again, but will only state the final result. We find namely,

$$t_2 = 137°{\cdot}43.$$

The equation (49) which serves to determine t_3 takes for this case the following form:

$$(57) \qquad g_3 = 26{\cdot}604 + 51{\cdot}515 \log \frac{T_2}{T_3}.$$

From this we obtain in succession the following approximate values.

$$\begin{aligned} t' &= 99°{\cdot}24. \\ t'' &= 101{\cdot}93. \\ t''' &= 101{\cdot}74. \\ t'''' &= 101{\cdot}76. \end{aligned}$$

The last of these values, from which the later ones would only differ in the highest decimals, we consider as the correct value of t_2, and apply it together with the known values of t_1 and t_0 to the first of equations (XVII). Thence we have

$$W = 31080.$$

If we calculate the work according to Pambour's equation (XII), attributing the same value to V, we find

$$W = 32640,$$

whereby however we must not take the values of B and b as in the machine without condenser from equation (29*b*), but from equation (29*a*) determined for machines with condensers.

57. In the same way, as is here indicated for the volume 1·5, I have also calculated the work for the volumes 1·2, 1·8, and 2·1. I have besides added also the following cases in order to be able to exhibit in one example, in a clear point of view, the difference which the different imperfections of the machine exert upon the quantity of work.

(1.) The case of a machine which has no injurious space, and in which, besides, the pressure in the cylinder during the influx is equal to that in the boiler, and the expansion is driven until the pressure has diminished from its original value p_1 to p_0. This, if we only assume that p_0 accurately represents the pressure in the condenser is the case to which equation (XI) refers, and which gives the greatest possible work for a given quantity of heat, if also the temperatures of absorbing and giving out heat are considered as given.

(2.) The case of a machine in which again no injurious space occurs, and the pressure in the cylinder is equal to that in the boiler, but the expansion does not take place as before completely, but only in the ratio of $e : 1$. This is the case to which equation (X) refers, only that there, in order to determine the quantity of the expansion, the change of temperature produced

by the expansion of the steam, was considered as known, while here, the expansion is determined according to the volume, and the change of temperature must first be calculated from this.

(3.) The case of a machine with injurious space and imperfect expansion, in which, of the former advantageous conditions only this one remains, that the steam in the cylinder during the influx exerts the same pressure as in the boiler, so that thus the volume has the least possible value. With this case, finally, are connected those already mentioned, in which also the lost advantageous condition is absent, inasmuch as the volume instead of the smallest possible value has other given values.

All these cases are also calculated according to Pambour's theory for the sake of comparison, with the exception of the first, for which equations (29*a*) and (29*b*) do not suffice, inasmuch as even that one of them which is determined for a less pressure can still only be applied up to $\frac{1}{2}$, or at the utmost downwards to $\frac{1}{3}$d of an atmosphere, while here the pressure is to diminish to $\frac{1}{4}$th of an atmosphere.

The numbers resulting from our equations for this first case, are as follows:

Vol. before expansion.	Vol. after expansion.	*W*
0·3637	6·345	50460

For all other cases, the results are embraced in the accompanying table, in which again the numbers which refer to the machine without injurious space, are separated from the others by a line. Only the numbers which hold good for the volume after the expansion are cited, because the values before the expansion are given, inasmuch as they in all cases are smaller in the ratio of $e:1$.

V	t_2	t_3	*W*	According to Pambour.	
				V	*W*
0·992	152°·22	113°·71	34300	1·032	36650
1·010	152°·22	113°·68	32430	1·032	34090
1·2	145 ·63	108 ·38	31870	1·2	33570
1·5	137 ·43	101 ·76	31080	1·5	32640
1·8	131 ·02	96 ·55	30280	1·8	31710
2·1	125 ·79	92 ·30	29490	2·1	30780

58. The quantities of work given in this table, as well as those of the former table for machines without condensers, refer to the kilogram of steam passing out of the boiler. We may, however, according to this, also easily refer the work to a unit of heat delivered by the source of heat, if we consider that, for every kilogram of steam, as much heat must be delivered as is necessary to heat the mass *l*, which is somewhat greater than one kilogram, from its initial temperature with which it enters the boiler, to the temperature which prevails in the boiler itself,

and at this last to convert a kilogram into steam, which quantity of heat may be calculated from the data already given.

59. In conclusion, I must add yet a few words on the friction, in which however I will confine myself to justifying my course in leaving the friction entirely disregarded in the equations hitherto developed, by showing that instead of introducing the friction into the first general expressions for the work as Pambour has done, we may bring it into calculation, according to the same principles, subsequently, which in fact has been done in the same manner also by other writers.

The forces which the machine has to overcome, when in action, may be distinguished in the following manner. 1. The resistance which is opposed to it from without, and the overcoming of which forms the useful work required of it. Pambour calls this resistance the load (*charge*) of the machine. 2. The resistances which have their origin in the machine itself, so that the work consumed in overcoming them is not externally useful. We comprehend all these last resistances under the name of the friction, although besides the friction, in the more narrow sense, other forces occur among them, particularly the resistances of the pumps belonging to the steam engine, with the exception of the one which feeds the boiler, and which has already been considered in what precedes.

Pambour introduces into calculation both kinds of resistances as forces which are opposed to the motion of the piston; and in order to be able to unite them conveniently with the pressure-forces of the steam upon both sides of the piston, he selects the notation in the same manner as this is done for the pressure of the steam, namely, so that the symbol does not signify the whole force, but the whole portion of it which comes upon the unit of surface of the piston. Let the letter R denote the load in this sense.

A still further distinction must be made in the case of the friction. The friction, namely, has not a constant value for every machine, but increases with the load. Pambour decomposes it therefore into two parts, that which is already present when the machine moves without load, and that which is first added by the load. With respect to the last, he assumes that it is proportional to the load. He accordingly expresses the friction referred to the unit of surface by

$$f + \delta . R,$$

in which f and δ are quantities which, it is true, depend upon the arrangement and dimensions of the machine, but which according to Pambour, are to be considered as constant for a particular machine.

We may now refer the work of the machine, instead, as heretofore, to the moving force of the steam, to these resisting forces,

since the negative work done by these must be equal to the positive work done by the former, since otherwise an acceleration or retardation of the motion would occur, which contradicts the supposition made, according to which the motion is to be uniform. The surface of the piston describes the space $(1-\varepsilon)V$, while a unit of weight of steam passes into the cylinder, and we therefore obtain for the work W the expression

$$W = (1-\varepsilon)\,V[(1+\delta).R+f].$$

The *useful* portion of this work, on the other hand, which, to distinguish it from the whole work, may be denoted by (W), may be represented by the expression

$$(W) = (1-\varepsilon)\,V.R.$$

If we eliminate the quantity R from this equation by means of the previous one, we have

$$(58) \qquad (W) = \frac{W-(1-\varepsilon)\,V.f}{1+\delta}.$$

With the help of this equation, we may deduce the useful work W from the whole work (W) as soon as the quantities f and δ are given, since the quantity V is to be supposed as known.

I will not here enter upon the manner in which Pambour determines these last, since the determination still rests upon too uncertain foundations, and the friction in general, is foreign to the particular object of this memoir.

TABLE CONTAINING THE VALUES OF THE PRESSURE p WHICH HOLD GOOD FOR STEAM, OF ITS DIFFERENTIAL CO-EFFICIENT $\frac{dp}{dt}=g$ AND OF THE PRODUCT $T.g$ EXPRESSED IN MILLIMETERS OF MERCURY.

t Centigrade.	p	Δ	g	Δ	$T.g$	Δ
40°	54·906	3·003	2·935	0·139	919	46
41	57·909	3·145	3·074	0·144	965	49
42	61·054	3·291	3·218	0·149	1014	50
43	64·345	3·444	3·367	0·155	1064	52
44	67·789	3·601	3·522	0·161	1116	55
45	71·390	3·766	3·683	0·167	1171	57
46	75·156	3·935	3·850	0·173	1228	59
47	79·091	4·112	4·023	0·180	1287	62
48	83·203	4·294	4·203	0·185	1349	64
49	87·497	4·483	4·388	0·193	1413	67
50	91·980	4·679	4·581	0·199	1480	69
51	96·659	4·882	4·780	0·207	1549	72
52	101·541	5·092	4·987	0·213	1621	74
53	106·633	5·309	5·200	0·221	1695	78
54	111·942	5·533	5·421	0·228	1773	80
55	117·475	5·766	5·649	0·237	1853	83
56	123·241	6·006	5·886	0·244	1936	87
57	129·247	6·254	6·130	0·252	2023	89
58	135·501	6·510	6·382	0·260	2112	93
59	142·011	6·775	6·642	0·269	2205	96
60	148·786	7·048	6·911	0·278	2301	100
61	155·834	7·330	7·189	0·286	2401	103

TABLE—CONTINUED.

t	p	Δ	g	Δ	$T.g$	Δ
62°	163·164	7·621	7·475	0·296	2504	107
63	170·785	7·922	7·771	0·305	2611	111
64	178·707	8·231	8·076	0·314	2722	114
65	186·938	8·550	8·390	0·325	2836	118
66	195·488	8·880	8·715	0·334	2954	123
67	204·368	9·218	9·049	0·344	3077	126
68	213·586	9·568	9·393	0·355	3203	131
69	223·154	9·928	9·748	0·365	3334	135
70	233·082	10·298	10·113	0·376	3469	139
71	243·380	10·680	10·489	0·387	3608	144
72	254·060	11·072	10·876	0·398	3752	149
73	265·132	11·476	11·274	0·410	3901	153
74	276·608	11·892	11·684	0·422	4054	159
75	288·500	12·320	12·106	0·433	4213	163
76	300·820	12·759	12·539	0·445	4376	168
77	313·579	13·210	12·984	0·458	4544	174
78	326·789	13·675	13·442	0·471	4718	179
79	340·464	14·152	13·913	0·484	4897	185
80	354·616	14·642	14·397	0·497	5082	190
81	369·258	15·146	14·894	0·511	5272	197
82	384·404	15·664	15·405	0·524	5469	202
83	400·068	16·194	15·929	0·538	5671	208
84	416·262	16·740	16·467	0·552	5879	214
85	433·002	17·299	17·019	0·577	6093	220
86	450·301	17·874	17·586	0·582	6313	227
87	468·175	18·463	18·168	0·597	6540	234
88	486·638	19·067	18·765	0·612	6774	240
89	505·705	19·687	19·377	0·628	7014	248
90	525·392	20·323	20·005	0·644	7262	254
91	545·715	20·975	20·649	0·660	7516	262
92	566·690	21·643	21·309	0·676	7778	269
93	588 333	22·328	21·985	0·694	8047	276
94	610·661	23·031	22·679	0·712	8323	285
95	633·692	23·751	23·391	0·728	8608	292
96	657·443	24·488	24·119	0·747	8900	300
97	681·931	25·243	24·865	0·765	9200	309
98	707·174	26·017	25·630	0·783	9509	317
99	733·191	26·809	26·413	0·787	9826	320
100	760·00	27·59	27·200	0·805	10146	328
101	787·59	28·42	28·005	0·840	10474	343
102	816·01	29·27	28·845	0·855	10817	350
103	845·28	30·13	29·700	0·865	11167	356
104	875·41	31·00	30·565	0·885	11523	367
105	906·41	31·90	31·450	0·915	11888	378
106	938 31	32·83	32·365	0·935	12266	388
107	971·14	33·77	33·300	0·955	12654	397
108	1004·91	34·74	34·255	0·975	13051	407
109	1039·65	35·72	35·230	0·990	13458	414
110	1075·37	36·72	36·220	1·010	13872	424
111	1112·09	37·74	37·230	1·030	14296	434
112	1149·83	38·78	38·260	1·060	14730	448
113	1188·61	39·86	39·320	1·080	15178	457
114	1228·47	40·94	40·400	1·100	15635	467
115	1269·41	42·06	41·500	1·125	16102	479
116	1311·47	43·19	42·625	1·150	16581	491
117	1354·66	44 36	43·775	1·170	17072	502
118	1399·02	45·53	44·945	1 185	17574	509
119	1444·55	46·73	46·130	1·220	18083	526
120	1491·28	47·97	47·350	1·245	18609	537
121	1539·25	49·22	48·595	1·260	19146	547

TABLE—CONTINUED.

t	p	Δ	g	Δ	$T.g$	Δ
122°	1588·47	50·49	49·855	1·290	19693	560
123	1638·96	51·80	51·145	1·315	20253	574
124	1690·76	53·12	52·460	1·335	20827	583
125	1743·88	54·47	53·795	1·365	21410	599
126	1798·35	55·85	55·160	1·400	22009	615
127	1854·20	57·27	56·560	1·415	22624	624
128	1911·47	58·68	57·975	1·430	23248	633
129	1970·15	60·13	59·405	1·470	23881	652
130	2030·28	61·62	60·875	1·500	24533	666
131	2091·90	63·13	62·375	1·520	25199	678
132	2155·03	64·66	63·895	1·550	25877	694
133	2219·69	66·23	65·445	1·575	26571	706
134	2285·92	67·81	67·020	1·600	27277	720
135	2353·73	69·43	68·620	1·630	27997	735
136	2423·16	71·07	70·250	1·670	28732	755
137	2494·23	72·77	71·920	1·685	29487	765
138	2567·00	74·44	73·605	1·710	30252	778
139	2641·44	76·19	75·315	1·750	31030	798
140	2717·63	77·94	77·065	1·770	31828	810
141	2795·57	79·73	78·835	1·810	32638	830
142	2875·30	81·56	80·645	1·835	33468	844
143	2956·86	83·40	82·480	1·865	34312	860
144	3040·26	85·29	84·345	1·895	35172	876
145	3125·55	87·19	86·240	1·920	36048	891
146	3212·74	89·13	88·160	1·960	36939	911
147	3301·87	91·11	90·120	1·990	37850	928
148	3392·98	93·11	92·110	2·015	38778	943
149	3486·09	95·14	94·125	2·045	39721	959
150	3581·23	97·20	96 170	2·085	40680	980
151	3678·43	99·31	98·255	2·120	41660	999
152	3777·74	101·44	100·375	2·140	42659	1012
153	3879·18	103·59	102·515	2·175	43671	1032
154	3982·77	105·79	104·690	2·220	44703	1054
155	4088·56	108·03	106·910	2·250	45757	1073
156	4196·59	110·29	109·160	2·270	46830	1085
157	4306·88	112·57	111·430	2·310	47915	1107
158	4419·45	114·91	113·740	2·345	49022	1127
159	4534·36	117·26	116·085	2·375	50149	1144
160	4651·62	119·66	118·460	2·410	51293	1165
161	4771·28	122·08	120·870	2·445	52458	1184
162	4893·36	124·55	123·315	2·490	53642	1209
163	5017·91	127·06	125·805	2·510	54851	1222
164	5144·97	129·57	128·315	2·545	56073	1244
165	5274·54	132·15	130·860	2·585	57317	1265
166	5406·69	134·74	133·445	2·620	58582	1286
167	5541·43	137·39	136·065	2·670	59868	1314
168	5678·82	140·08	138·735	2·685	61182	1326
169	5818·90	142·76	141·420	2·725	62508	1348
170	5961·66	145·53	144·145	2·765	63856	1372
171	6107·19	148·29	146·910	2·795	65228	1390
172	6255·48	151·12	149·705	2·830	66618	1412
173	6406·60	153·95	152·535	2·880	68030	1440
174	6560·55	156·88	155·415	2·920	69470	1464
175	6717·43	159·79	158·335	2·935	70934	1476
176	6877·22	162·75	161·270	2·980	72410	1502
177	7039·97	165·75	164·250	3·025	73912	1529
178	7205·72	168·80	167·275	3·060	75441	1550
179	7374·52	171·87	170·335	3·090	76991	1570
180	7546·39	174·98	173·425	3·140	78561	1599
181	7721·37	178·15	176·565	3·170	80160	1619

TABLE—CONCLUDED.

t	p	Δ	g	Δ	$T.g$	Δ
182°	7899·52	181·32	179·735	3·205	81779	1642
183	8080·84	184·56	182·940	3·255	83421	1670
184	8265·40	187·83	186·195	3·280	85091	1688
185	8453·23	191·12	189·425	3·320	86779	1714
186	8644·35	194·47	192·795	3·370	88493	1743
187	8838·82	197·86	196·165	3·400	90236	1763
188	9036·68	201·27	199·565	3·445	91999	1792
189	9237·95	204·75	203·010	3·480	93791	1814
190	9442·70	208·23	206·490	3·515	95605	1837
191	9650·93	211·78	210·005	3·550	97442	1861
192	9862·71	215·33	213·555	3·595	99303	1889
193	10078·04	218·97	217·150	3·645	101192	1919
194	10297·01	222·62	220·795	3·675	103111	1941
195	10519·63	226·32	224·470	3·715	105052	1966
196	10745·95	230·05	228·185	3·750	107018	1991
197	10976·00	233·82	231·935	3·795	109009	2020
198	11209·82	237·64	235·730	3·840	111029	2048
199	11447·46	241·50	239·570	3·885	113077	2077
200	11688·96		243·455		115154	

ART. VIII.—*On the Agency of the Gulf Stream in the Formation of the Peninsula and Keys of Florida;* by JOSEPH LECONTE, M. D., Prof. Natural Sciences, University of Georgia.

Read before the American Association for the Advancement of Science at Albany, August, 1856.

IN the winter of 1851, and during the months of January and February, I enjoyed the rare opportunity of visiting and examining the keys and reefs of Florida, in company with Professor Agassiz. I then and there became deeply interested in a subject which has continued to occupy my thoughts from time to time until now—viz., the mode of formation of the peninsula of Florida.

Until the time referred to, nothing definite was known of the geology of Florida, but it was supposed to consist of a southward prolongation of the eocene of Georgia and Alabama, and its shell limestone to bear some general resemblance to the white limestone of these states. But the observations of Professor Tuomey during the summer of 1850,* and the more full and careful observations of Professor Agassiz during the following winter†, brought to light the remarkable fact, that the keys and the larger portion of the peninsula of Florida are of recent origin, and as far as could be examined, the work of corals still living in the vicinity, and still engaged in the work of extension; that they are in fact, superficially at least, the result of the growth of successive coral reefs concentrically arranged, one outside of the other, from north to south. My object in the present paper will be to show that coral agency alone is not sufficient to account for the phenomena, but that there has been another and still more powerful

* This Journal, [2], vol. i, p. 390. † Report of Coast Survey for 1851, p. 145.

agent at work, preparing the ground and laying the foundation for these builders, and that this agent has been the Gulf Stream. A clear understanding of the subject renders necessary a succinct account of the views of Tuomey and Agassiz.

Fig. 1 represents the peninsula of Florida with its keys and reefs; *a b*, the southern coast; *a' b'* the line of keys stretching from Cape Florida to the Tortugas; *a'' b''* the living reef; *G S S* the Gulf Stream sweeping close around the reef; *d*, Tampa Bay; *d'*, Charlotte's Harbor; *d''*, Chatham or Gallivari Bay.

The southern coast *a b*, is elevated about 12–15 feet above the level of the sea; but within this line there is *e*, the everglades, an extensive swamp only a few feet above the sea level, covered with fresh water and dotted over with small islands called Hammocks. Between the southern coast *a b*, and the line of keys *a' b'*—a distance at the point *b* of about 40 miles—the water is very shoal, navigable only for the smallest fishing craft, and dotted over with small low mangrove-islands. Between the line of keys *a' b'* and the living reef *a'' b''* there is a ship-channel 3 or 4 fathoms deep and 5 or 6 miles wide. Beyond the reef *a'' b''* the sea bottom slopes rapidly into the almost unfathomable abyss of the Gulf Stream.

Now, we have the best evidence that the Everglades, the southern coast and the keys are all formed by coral agency. The evidence is briefly as follows:

It is a well known fact that corals cannot grow above the surface of the water. The islands, therefore, so commonly found on coral reefs cannot be formed by the agency of these animals alone, but are due to the violent action of waves breaking off huge coral heads and overturning coral trees, bearing them from the outer and more exposed side, and piling them on the middle and inner side of the reef. These coral boulders form the nucleus around which cluster smaller fragments and coral sand; the whole is then firmly cemented by carbonate of lime in solution in the sea water, and the island thus formed is finally covered with vegetation and inhabited by animals and man. The whole embryonic development, if I might use the expression, of coral islands may be observed upon the keys and reefs of Florida. On the outer or living reef a few have commenced to form only a few years ago, and exist as yet only in the form of isolated boulders of dead coral, and not yet dignified with the name of keys. Others are formed of similar boulders, mingled with smaller fragments, and coral sand, and firmly cemented by carbonate of lime; but the large boulders are still conspicuous above the surrounding land, though immovably fixed. Still others are so covered with coral sand that the boulders are not observable, except by excavation or by examination of the outermost portion of the island towards the sea. The coral sand is always affected with the cross and oblique stratification so common in materials exposed to the violent action of the waves. All the islands on the outer reef are very small, of very recent origin, (some only a few years old,) and, therefore, as yet entirely barren.

The examination of the larger and older inhabited islands of the line of keys prove beyond question that they have been formed in a similar manner. We have here also the same coral boulders, mingled with smaller fragments and coral sand, and the whole firmly cemented into solid rock, the same cross and oblique stratification indicating the former action of waves on an exposed shore. The boulders here also sometimes stand above the surrounding cement exposed in their superior portions, as at Key Vaca; and at others completely covered with coral sand, as at Key West, and most other keys. This exposure of the larger boulders above the surrounding cement in which they are firmly fixed led Tuomey into the error of supposing that they were the prominent points of the original reef, elevated above the sea level by igneous agency, and that the keys were formed by igneous rather than by aqueous agency. That such is not the case is proved by more attentive examination and comparison with the smaller keys of the outer reef. There can be no doubt, there-

fore, that the line $a'b'$, marks the position of a former reef changed into keys by the action of waves alone.

It has been pointed out by Tuomey, and proved by Agassiz, that in a similar manner, the southern coast of Florida was the position of still another and earlier reef. The character of the rock is the same as that of the keys of the main range, or of the smaller ones on the living reef. Here also Tuomey has seen, as he supposes, the evidence of elevatory forces, while Agassiz sees nothing but the action of the waves.

There seems to be no reasonable doubt, therefore, that at some former period, the northern shore of the everglades was the position of the southern coast, and at the same time the present southern coast was the position of a reef. The general sequence of changes has been as follows: The reef $a\,b$ became gradually converted into a line of keys, and was finally added to the mainland, and the shoal water became the everglades, and its mangrove islands, the hammocks which overdot this swamp. In the meantime, that is, while the present southern coast was still a line of keys, another reef was formed farther out. This became in time converted also into a line of keys, and will eventually be added, in its turn, to the main land and become the southern coast—the shoal water between $a\,b$ and $a'\,b'$, with its mangrove islands, becoming another everglade, with its hammocks. In the meantime, still another reef has been formed still farther out, (viz: the present or living reef,) and upon this, too, the process of key formation has already commenced. Any farther extension, however, in this direction, by the growth of still another reef, seems precluded by the proximity of deep water. Standing upon the reef, the blue waters of the Gulf Stream are distinctly seen at the distance of but a few hundred yards. Thus it appears that not only the keys, but the mainland of Florida, certainly as far north as the northern shores of the everglades, has been formed superficially, at least, by coral agency. The evidence in favor of a similar origin for that portion of the peninsula lying north of this line is less abundant, and perhaps less conclusive, and yet we have every reason to believe that the greater portion of this also was formed in a similar manner. Although the geology of this part has not, as far as I know, been examined by any one capable of deciding definitely as to what portion of the peninsula is tertiary and what is recent coral formation, yet specimens of coral rock precisely similar to that of the southern coast and keys, sent to Professor Agassiz from the shores of Lake George and other parts of Florida as far north as St. Augustine, leave no doubt that on the eastern coast at least the coral formation extends as far north as that ancient city. I have myself a fragment of Meandrina from the neighborhood of St. Augustine, undistinguishable from fragments which may be picked up any-

where upon the keys. The western shore is still less known, but Conrad and Tuomey state it as their opinion that the bluffs of Tampa are Eocene Tertiary. Supposing this to be a fact, though it is still problematical, then all that portion of the peninsula lying south of the line $c\,d$ is almost certainly of coral origin and formed in the manner already indicated, viz: by the growth of successive reefs. As to the position of these supposed reefs we know absolutely nothing. The position of the lines $c\,d$, $c'\,d'$, $c''\,d''$ has been merely suggested by the succession of bays which indent the western coast. May they not all have been formed like Chatham Bay, by the imperfect filling up of the shoal water, separating successive reefs from the mainland?

Such is a brief account of Professor Agassiz's views concerning the mode of formation of the peninsula and keys of Florida. I will now attempt to show that coral agency alone is not sufficient for this purpose, and that to suppose so would violate all probability, and contradict all that we know of the laws which govern the growth of these animals.

It is a well known fact that reef-building corals cannot grow at a greater depth than from ten to twelve fathoms. It is also certain that they cannot grow above the surface of the water at low tide. Thus they are limited in a vertical direction to a space of about sixty or seventy feet. Unless there is subsidence of the sea bottom, therefore, it is impossible that a reef should be more than sixty or seventy feet thick. To this may be added in the case of coral islands from ten to fifteen feet for material accumulated above the sea level by the agency of waves. If there is no subsidence, therefore no coral formation can be more than eighty feet in thickness. Now, nothing can be more certain than that there has been no subsidence whatever of the sea-bottom upon which grow the reefs of Florida, for otherwise the extension of the peninsula by means of coral agency would have been impossible. It necessarily follows, therefore, that the coral formation of Florida, whether upon the main land, or upon the keys, or upon the living reef, can no where be more than seventy or eighty feet thick. In other words, it is evident that Florida and the keys are only faced or encrusted with coral formation. If, then, corals have been the only agents in this work, if the sea bottom has remained substantially unchanged during the whole time the coral work was progressing, it is evident that the sea, for the enormous distance of five degrees of latitude, viz: from St. Augustine to the present reef, was nowhere more than sixty or seventy feet in depth, and Florida must have been represented by a tongue of shoal water three hundred miles in length—a circumstance possible, certainly, but so improbable that it behoves those who maintain the theory that coral alone has formed the peninsula to account for it.

But even if we admit the probability of such a condition of things, we do not get rid of the main difficulty; for in that case there is no apparent reason why the coral should not grow over the whole area at the same time, as an immense coral forest, instead of in the form of successive reefs. In a word, the fact that the corals grew in the form of successive reefs, concentrically disposed from north to south, proves, as it seems to me, incontestibly, that the conditions necessary for coral growth have also been progressively formed in the same direction. The horizontal extension of coral through so great a space proves also the progressive extension of necessary conditions; in other words, it proves that the sea bottom has been gradually rising from the north towards the south.

Such a gradual rising of the sea bottom may be attributed to one of two causes, viz:—first, gradual elevation by igneous agency; and second, filling up by sedimentary deposit. As we have already seen, Professor Tuomey has thought that there are evidences of such igneous elevation upon the keys as well as upon the main land; but the more careful observations of Prof. Agassiz have satisfactorily explained these deceptive appearances, so that we may now say with confidence that there is not the slightest evidence of such elevation, but much evidence to the contrary. Neither the mainland nor the keys are anywhere higher than may be accounted for by the action of waves, viz: from ten to fifteen feet, and it is inconceivable that this elevation should have gone on progressively preparing ground for the growth of successive reefs, without in the slightest degree affecting the contiguous and recently formed land. But this is precisely the mode of action of sedimentary deposit. Sediment cannot, of course, affect anything but the sea bottom. It is to sedimentary deposit, therefore, that I attribute the gradual rising of the sea bottom from north towards the south, which, as we have seen, forms the necessary condition for the horizontal extension of coral reefs through so great a distance.

Having thus shown that sedimentary deposit is almost absolutely necessary for the explanation of the southward extension of the reefs of Florida, let us attempt to prove that such deposit has in fact taken place under the influence of the Gulf Stream.

It is a well known law of currents bearing sediment, that if from any cause their velocity is checked, they deposit a portion of their sediment upon the bottom; but if, on the contrary, their velocity is increased, they abrade their beds and banks. If, therefore, the velocity of a stream is greater on one side than on the other, abrasion will take place on the former, and deposit on the latter. Now, if such a stream, bearing sediment, make a sweep or curve, the velocity will always be greater on the outer, and least on the inner side of the sweep. Hence there must

necessarily be abrasion of the outer bank and deposit upon the inner. Thus, in proportion as the outer curve extends by abrasion of the outer bank, the inner curve will extend also by deposit, and the tongue of land around which the sweep is made will grow longer and longer. If this tongue be cut away by artificial means, so as to convert this portion of the stream into a lake, around the outer margin of which sweeps the current, the still water within the sweep will become more and more shoal, until it is again converted into a tongue of land. Now, this is necessarily true under all circumstances. It makes no difference whether the stream runs between banks of solid matter or between banks of still water. If a stream, leaving sediment, runs through still water, making a sweep or curve, the sediment must deposit principally upon the inner side of the curve, making shoal water at this part: the curve will extend, and the shoal water will extend in the same proportion.

Now, the Gulf Stream is such a current. The sweep which it makes around the point of Florida is seen in fig. 1. If, therefore, the Gulf Stream bears any sediment, the conclusion seems irresistable that the sweep of the curve has been increasing with the course of time, and that the tongue of land within the curve, viz: the Peninsula of Florida, has been extending, *pari passu*, by means of sedimentary deposit. Or, even supposing that the position of the Gulf Stream has always been the same as at present, and that Florida was once represented by a tongue of still water within the curve, this tongue of still water must have become more and more shoal by sedimentary deposit. I repeat, then, that upon any conceivable theory as to the position of the Gulf Stream, whether its curve has been increasing or has been always the same as at present, if it carries sediment, according to the laws of currents, there must have been a progressive shoaling of water within the curve from north toward the south, and, consequently, a progressive formation of the conditions necessary for the growth of coral, and their extension in the same direction. What evidence, then, have we that the Gulf Stream does indeed carry sediment?

The Gulf Stream is supposed to be a continuation of the great equatorial current which, stretching across the Atlantic from the coast of Africa, strikes upon the wedge-shaped point of the eastern coast of South America, and divides into a northern and southern branch. The northern branch, uniting with the water of the Amazon and Orinoco, runs along the coast of South America, through the Caribbean Sea, under the name of the Caribbean current, enters and receives strength in the Gulf of Mexico from which emerging it sweeps round the point of Florida and along the coast of the United States, on its way to the coast of Europe.

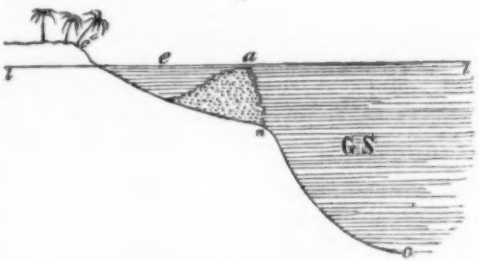
Now, is it possible that a stream which washes so many shores, which runs through seas into which are poured such enormous quantities of sediment, brought down by the largest rivers in the world, is it conceivable that such a stream should carry no sediment? On the contrary, it is well known that the sediment both of the Amazon and Orinoco rivers is carried by this stream, and is distinctly traceable for several hundred miles. Much of it is doubtless deposited along the coast and in the Caribbean Sea, but "according to Humboldt, much sediment is carried again out of the Caribbean Sea into the Gulf of Mexico."* Into this same Gulf is also poured the enormous amount of sediment brought down by the Gulf rivers, especially by the Mississippi. Out of this Gulf, the waters of which are thus highly charged with sediment, comes the Gulf Stream on its way round the point of Florida. If, then, this stream mingles at all with the waters of the Gulf rivers, it must necessarily carry sediment. That it does thus mingle, is proved by the fact mentioned by Lyell,† that drift timber from the Mississippi is carried by this stream to the shores of Iceland and Europe. Now, unless we suppose that the whole of this sediment is deposited in the Gulf, it must reach, and, by the law of currents already insisted on, be deposited, much of it, on the point of Florida. But we have the best reason for believing that it is not all deposited in the Gulf. The distance from the mouth of the Mississippi to the Tortugas is about five hundred miles. Taking the velocity of the Gulf Stream through the Gulf of Mexico at three miles per hour, it would traverse this distance in about seven days. Now, the finest sediment will not sink more than‡ an inch in an hour; but supposing the mud brought down by the Mississippi sinks at the rate of one foot per hour, in seven days it would sink 168 feet. But the depth of the Gulf of Mexico and the Gulf Stream is much greater than this. Therefore, fine sediment from the Mississippi would reach the point of Florida, and what was not deposited there would even be carried much farther on. We have farther evidence of this in the soundings made by the Coast Survey off the eastern coast of Florida, and the nature of the bottom which has thus been brought up. It can scarcely be doubted for a moment that the banks of sand and mud, found in the bed and on the margin of the Gulf Stream off the eastern coast of Florida, were deposited there by that stream.

But it will be objected that the water of the Gulf Stream is remarkable for its transparency. This objection, however, will entirely disappear when we consider the difference between river and ocean currents. The former are of slight depth, and run over rough bottoms, and between banks possessing many ine-

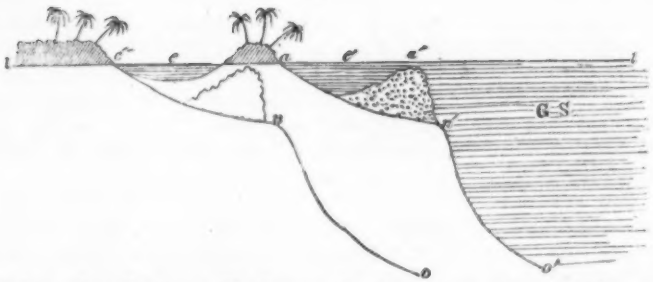
* Lyell's Prin., p. 328. † Lyell's Prin., p. 721. ‡ Lyell's Prin., p. 328.

qualities of surface, and offering, therefore, much resistance; hence are generated partial currents upward and downward, to the right and left, which thoroughly mix, as if by a sort of ebullition, the waters of the river. It is impossible that matters in suspension or solution should exist in one part and not in another; *e. g.*, that sediment should be carried by the deeper strata, while the superficial strata remain transparent. But with oceanic streams the case is quite different. Their great depth, and the fact that they are bounded on all sides by still water; in other words that they run over perfectly even beds and between perfectly smooth banks, causes them to flow without the slightest agitation, without the ripples and inequalities which mark the currents of rivers. The currents of oceanic streams, therefore, do not in the slightest degree interfere with the natural subsidence of materials in suspension. They are equally as favorable to subsidence as perfectly still water. The surface transparency of the Gulf Stream, therefore, forms no objection whatever to the supposition that it carries sediment in the deeper strata. Such sediment would necessarily sink beyond observation in the course of a few hundred miles.

2.

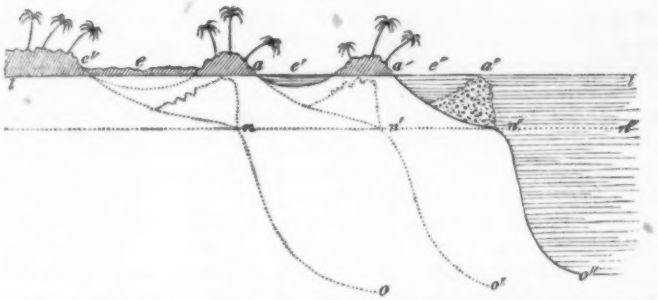


3



After what I have said above, the bare inspection of the accompanying figures will explain the application of the theory to the formation of the Peninsula and Keys.

4.



Figs. 2, 3 and 4, are ideal sections through the middle of the Peninsula and Keys, along the line *p p* fig. 1, representing the different stages of the process; *l l* the sea level, *G S* the Gulf Stream, *c'' a a' a''* sections of the lines *c''d'' ab a'b' a''b''* of fig. 1. Fig. 2 represents the condition of things at some period anterior to the present elongation of the Peninsula, *e. g.*, when the southern coast was in the position of the northern shores of the everglades, *c'' n o* the supposed configuration of sea bottom at that time. At the point *n* at the depth of 60 feet would form a reef, *a*, leaving a ship channel *e* between the coast and reef. Fig. 3 represents the condition of things when the sea bottom by sedimentary deposit had advanced to *a n' o'*. The reef, *a*, has become now a line of Keys, the ship channel *e* has become shoal water dotted over with mangrove islands, (not here represented) and another reef, *a'*, has formed at the limiting depth of 60 feet, leaving another ship channel *e'* between the reef and the Keys. Fig. 4 represents the condition of things when the sea bottom had advanced to *a' n'' o''*. Now *a*, the line of Keys of the last figure, has become the southern coast, the former shoal water *e* has become the Everglades, *a'*, the reef of last figure has become a line of Keys and its ship channel *e'* shoal water, and at the limiting depth of 60 feet still another reef, *a''* viz., the present living reef is formed with its ship channel *e''*. This figure therefore represents the present condition of things.

It is evident that if this theory be correct, and no insuperable obstacle is interposed, the Gulf Stream may continue to move its bed, and the point of Florida to extend almost indefinitely. But such an obstacle is interposed in the island of Cuba. The Gulf Stream cannot move much beyond its present position, nor Florida extend beyond the position of the present reef, except at the expense of Cuba and the Bahama Banks. Cuba can never be annexed by any natural agency, whether coral or current.

Or even supposing (as I have already done) that the position of the Gulf Stream has always been the same as at present, and that the peninsula of Florida was originally represented by a tongue of still water, yet, substantially, the same changes would necessarily have occurred.

It is evident that as the point of Florida approached the Gulf Stream, the slope of the bottom would become steeper, and therefore the limiting depth would be attained at a shorter distance from shore, the consecutive reefs would be formed nearer and nearer to one another, and the intervening ship channels would become narrower.

5

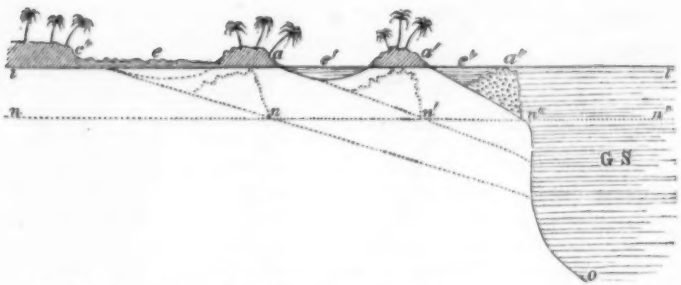


Fig. 5 is an ideal section showing the succession of changes which would occur on such a supposition. The letters represent the same things as in figs. 2, 3, and 4.

Inspection of fig. 1 shows that this has actually been the case at least for the last three reefs.

We have chosen to trace this process only as far as the northern shore of the everglades, because thus far we have the most indisputable evidence of the recency of the formation. But in the same manner we might carry it still farther back in time and northward in space, and represent the successive reefs by which the superficial portion of the rest of the peninsula was formed.

There is one other fact of great importance, and otherwise inexplicable, which receives a ready explanation upon this theory, and which I think, therefore, is strongly confirmatory of its truth. I allude to the fact that the successive reefs are found at some distance from one another; in other words, that the peninsula is formed by a succession of barrier reefs, instead of a continuous southward growth of fringing reef. The reefs of Florida are in some respects entirely peculiar. Barrier reefs have heretofore been considered as always the result of subsidences of the sea bottom, and are invariably looked on as the sign of such subsidence. But in Florida we have barrier reefs where it is certain there has been no subsidence. We have here, therefore, a new form of barrier reef. This important fact did not, I am sure, escape the attention of Professor Agassiz, for my own attention was first drawn to it by him; but I have seen no publication in which he has alluded to the fact, nor as far as I know, has he ever attempted or even thought of a probable explanation. The explanation which I would offer is as follows:—

It is a well known condition of coral growth that the sea water must be pure and transparent. Corals will not grow, therefore, on muddy shores, or in water upon the bottom of which sediment is deposited. Now, it must be borne in mind that while the Gulf Stream bears sediment in its deeper strata, it is superficially transparent, and we have already shown that

this must of necessity be the case with ocean streams. Suppose, then, that the matter held in suspension by the waters of the Gulf of Mexico be of such a degree of fineness that it sinks to the depth of sixty feet by the time it reaches the point of Florida. It is evident that the sea bottom within the curve cannot rise by deposit above this level, for all the sediment is below. A stream bearing sediment in all its strata from bottom to top—such as a river, for instance—will make land within the curve, but an ocean stream will only make shoal water within the curve. In the case supposed, where the bottom of the shoal rises to within sixty feet of the surface, it will cease to receive deposit, and the water will remain perfectly transparent. Here, then, it would seem we have the conditions necessary for coral growth. It must be recollected, however, that upon sloping shores, with mud bottom—such as we have supposed always existed at the point of Florida—a fringing reef cannot possibly form, for the water is rendered turbid by the chafing of waves against the mud bottom; but at some distance from shore, that is, where the depth of sixty or seventy feet is attained, and where the bottom is unaffected by the waves, the conditions favorable for coral growth would be found. Here, therefore, would be formed a barrier reef, limited on one side by the muddiness, and on the other by the depth of water.

It is evident then that the Peninsula and Keys of Florida have been the result of the combined action of at least three agencies. First, the Gulf Stream laid the foundation; upon this, corals built up to the water level; and finally the work was completed by the waves. Fig. 4 illustrates the relative importance of these agencies. All below the line $n\,n'\,n''\,n'''$, even to the bottom of the Gulf Stream, is due to the agency of that stream; all between the line $n\,n'\,n''\,n'''$ and the line $l\,l$ to the agency of corals and above the line $l\,l$ to waves.

I have said that a stream running through still water and making a curve would deposit most of its sediment on the inner side of the curve. This is certainly true; but it is a more general expression of the truth to say that a stream running through still water will deposit sediment on both sides, just where it comes in contact with the still water, and is retarded by it. It would do so for the same reason that rivers which habitually overflow their banks form natural levees on either side where the rapid current of the river comes in contact with the comparatively still water of the river swamp. It is well known that the natural levees of the Mississippi continue out to sea in the form of submarine banks, evidently formed by the checking of the velocity of the current on either side by contact with the still water of the Gulf. If the current is straight the deposit on

both sides will be equal, and thus the stream will form banks for itself. If the stream is curved the deposit will be mostly on the inner side of the curve, as already said. Is it not probable that the Bahama banks, or at least that portion of them that lie to the east of Florida, may have been formed to a great extent in the same way? That while the peninsula of Florida has been made on one side, the Bahama banks have been made on the other? It will be observed that the great Bahama banks lie off the eastern coast of Florida, and that the Gulf Stream runs in a narrow channel between them. At the point of Florida the deposit would, of course, be on the inner side of the curve, and would go, therefore, mostly or entirely to the extension of that peninsula; but after the stream turns northward and becomes nearly straight the deposit would be also on the other side, and thus probably have originated these banks. Even if we suppose, as is most probable, that there originally existed in this position islands or submarine hills, which turned the stream around the point of Florida, these have doubtless been greatly modified and extended by sedimentary deposit. Probably also even the general form of the Alantic bottom—very sloping until the Gulf Stream is reached, and then plunging rapidly into an almost unfathomable abyss, forming a deep bed for that stream—may, to some extent, at least, be accounted for in a similar manner, for certain it is that a stream running through still water, no less than one running over land, will make its own bed; only in the latter case by abrasion it cuts out its own channel, while in the former, by deposit, it builds its own banks.

This property of ocean streams, viz.: that they form banks or ridges where they come in contact with still water, affords a possible and, as it seems to me, even a probable explanation of certain remarkable peculiarities of sea bottom, brought to light by recent soundings across* the Gulf Stream. Commencing at Charleston, the bed of the ocean slopes at first very gently, so that at a distance of 50 miles from shore it attains only the depth of 20 fathoms, and then very rapidly, so that in 25 miles more it sinks to the depth of 700 fathoms or more. At the additional distance of another 25 miles (*i. e.* 100 miles from shore), at the depth of 300 fathoms is found a ridge rising from unfathomable depths on one (coast) side, and 1,500 feet above the hollow on the other side. At the distance of a little more than 20 miles more is found another ridge 500 feet high, followed by still another hollow. Further observation shows that the Gulf Stream is divided into longitudinal bands or streams of warm and cold water, see fig. 1, and that the warm bands correspond to the bottom and the cold bands to the ridges.

* Proc. Am. Assoc., Washington Meeting, p. 140.

Now, these ridges and hollows may be conceived to have been formed in either of two ways, viz.: by igneous or current agency. As upon land valleys are formed either by igneous or aqueous agency, *i. e.*, may be valleys of elevation or valleys of erosion; so also in the sea, ridges may be formed by igneous or current agency, *i. e.*, may be ridges of elevation or ridges of deposit. In either case there would be conformity between the direction of the ridges and the direction of the current, only in the one case the current would conform to the ridges, and in the other the ridges would conform to the current.

In order to account for these ridges by this current theory, the only supposition which it is necessary to make is, that there exists in the bed of the Gulf Stream somewhere to the southward of the Charleston section, *i. e.*, at the southern extremity of the ridges, two or more submarine peaks or mountains—possibly a spur of the Bahama chain. If two such peaks existed in this position, and rose so high as to part the lower strata of the Gulf Stream, there would evidently be formed bands of comparatively still water to the northward, and as evidently there would be lines of deposit determined by the still water, and the necessary result would be the ridges discovered by the Coast Survey. We see the same thing on a small scale in river currents. Every obstacle which parts the current determines the position of a sand ridge on the lower side of the obstacle, and in the direction of the current. There is this remarkable difference, however, between river and ocean currents—that while in the case of rivers the parted current quickly closes again, and the resulting ridge is therefore very short, in ocean currents, such as the Gulf Stream, the space between the two parts would be quickly filled by the cold water of the ocean. The parted current would have, therefore, no disposition to coalesce, but would continue as bands of warm Gulf water separated by bands of cooler and stiller Atlantic water, and the resulting ridges would therefore continue for great distances. I know not whether there have been any observations to test the comparative velocity of the warm and cool bands, but it seems to me that on any conceivable theory as to the formation of the ridges the velocity of the cool bands would be less.

Now, though it may be impossible in the present condition of science to determine with absolute certainty whether these ridges were formed previous to the existence of the Gulf Stream, by igneous agency, or whether they have been formed since by the sediment carried by the stream itself, yet, when we recollect that all the other peculiarities of the Gulf Stream and the contiguous sea bottom are mainly referable to sedimentary deposit; when we reflect how simple and natural is the only supposition required, and how easily it explains all the phenomena, particularly the

cold bands, which seem inexplicable on any other theory, unless we suppose the existence of lateral currents, it seems to me that the weight of probability will strongly incline to sedimentary deposit as the cause also of these ridges. In fact, everything about the Gulf Stream seems to point to the conclusion that it has been the architect of its own curves, its own banks, and its own configuration of sea bottom.

There is one other conclusion which, though not connected with any particular theory of the formation of Florida, is, nevertheless, naturally suggested by the subject of this paper. We have seen that the peninsula of Florida has been progressively advancing towards Cuba as a fixed point, and the Gulf Stream has been becoming more and more narrow. If, therefore, as is probable, the quantity of water carried by the Gulf Stream has remained constant, it follows that the velocity with which the stream emerges from the Straits of Florida, and therefore the distance to which it penetrates the still water of the Atlantic, has been progressively increasing. Now, unless there has been some very remarkable change in the direction of this current, it necessarily follows that its warming influence upon the European continent has also been progressively increasing. Have we not here, if not a sufficient cause, at least one of the true causes of that great change which we know has taken place in the climate of Europe since the glacial period?

Thus we see that the advancing point of Florida has been progressively warming the climate of Europe, and thus, perhaps, controlling the destinies of the human race. Can we conceive a more beautiful instance than this of that sympathy which exists between the most distant portions of our globe, and which binds all its members together in one organic whole?

ART. IX.—*On Screw-Propellors;* by W. ROGERS HOPKINS, Assistant Professor, U. S. Naval Academy, Annapolis.

Is it not strange that while in heavy machinery on land revolving at high velocities no difficulty is found in preventing heating in the journals, from friction, that few propellors are afloat at sea that have not suffered seriously from this cause? We hear of vessels on both sides of the Atlantic, mercantile and armed, that are retarded by the heating and wearing in the stuffing boxes and bearings of their shafts.

It appears to the writer that the causes for this can be easily explained, and the effects modified if not prevented. The heating of the bearings inside a vessel results from one cause: the wear and heat in the stuffing box and outside journals result from totally different causes.

In most cases the machinery of a steamship is placed in the centre of the vessel, and thence motion is carried to the propellor blades by a long shaft rigidly connected. If the frame of the vessel springs at all by the motion of the sea, the shaft is thrown out of line and must consequently heat. To remedy this the shaft should be allowed some *play* in the couplings where the *lengths* of the shaft are joined together.

But it is the wearing of the journals and bearings outside the vessel that is most prejudicial, most frequent, and most difficult of repair. One cause of this wear is that the blades are not made smooth and not balanced, so that the centre of rotation and the centre of gravity do not coincide. No machinery in revolving works well under these circumstances.

But the most important disturbing cause is the following. The propellor blades of a vessel on leaving port are set in motion in a plane at right angles to the vessel's keel. The propellor blades tend to "persist" in this plane, and the greater their momentum the greater their resistance to any cause tending to draw them from this plane. But the motion of the vessel is a constant disturbing cause, and in resisting the motion of the vessel the revolving propellor presses with great force on the bearings.

Suppose, as in some vessels, the propellor (blades and hub) to weigh fifteen tons. Propellors of this size have their centres of oscillation moved at the rate of thirty-six feet per second when in full action. We have then a weight of fifteen tons moving at thirty-six feet per second, to be deflected from its line of action whenever the vessel rises or falls. The wear caused by this action has been attempted to be overcome by putting wooden linings in bearings; how far successfully has yet to be shewn.

It would undoubtedly be better to remove the cause than to remedy the effects. It seems to the writer that the cause may be easily removed by simply so arranging the propellor blades (or the frame in which they are mounted), that the propellor blades can keep in the original plane of rotation however the vessel may move in a sea way. The plans for effecting this are not easily explained without drawings. But means of so arranging the propellor blades that they will keep vertical however the vessel may move will occur to most persons acquainted with machinery.

Art. X.—*Statistics of the Flora of the Northern United States;* by Asa Gray.

[Continued from vol. xxii, p. 232.]

The Catalogues of the alpine and subalpine species of our Flora of the Northern States, given on pp. 230 and 231, in the former part of this communication, are found to be very imperfect, through some unaccountable omissions. They are here reproduced in a corrected form.

1. *List of Phænogamous Species found only in our small Alpine Region.*

Cardamine bellidifolia.	Arctostaphylos alpina.
Viola palustris.	Cassiope hypnoides.
Silene acaulis.	Phyllodoce taxifolia.
Sibbaldia procumbens.	Loiseleuria procumbens.
Potentilla frigida.	Rhododendron Lapponicum.
Epilobium alpinum, var. majus.	Veronica alpina.
Saxifraga rivularis.	Castilleia septentrionalis.
Saxifraga stellaris, var. comosa.*	Diapensia Lapponica.
Gnaphalium supinum.	Oxyria reniformis.
Nabalus Boottii.	Betula nana.
Nabalus nanus.	Salix phylicifolia.
Vaccinium cæspitosum.	*Salix Uva-Ursi.*

* This is a recent discovery, on Mount Katahdin, Maine, by Joseph Blake, Esq. Pursh had given *Saxifraga stellaris* as a native of Canada, but this was hardly credited: it has long been known from Labrador. But the *S. foliolosa*, which is plainly a state of *S. stellaris*, was known in this country only from the Arctic Islands, and from Sitcha on the northwest coast. With this species I replace *Dryas integrifolia* of the former list, which I think cannot have been really found on the White Mountains or elsewhere within our limits: Pursh must have mistaken something else for it in Prof. Peck's herbarium, as well as for *Alchemilla alpina*. Moreover, it is wrongly marked in the Manual of Bot. N. States, as also European.

The following points likewise need correction:

On p. 207, line 24, the Alleghany Mountains are said, in round numbers, to rise to an elevation of about 6300 feet. My excellent and accurate friend, Professor Guyot, informs me that the results of his barometrical measurements made during the past summer assign to the Black Mountain an elevation of fully 6710 feet above the level of the sea. Yet the summit was covered with trees (mostly *Abies Fraseri*), which, however, are now being cut away.

Page 217, et seq. The following additions may be made to the table, in their proper places. To the second column, i. e. Extra-European genera of E. North America, *Phascolus* and *Aralia*. To the fourth column, of temperate E. Asia, including the Himalayas, *Elodea*, *Stylosanthes*, *Phaseolus*, *Galactia*, *Amphicarpæa*, *Centrosema*, *Itea*, *Fothergilla*, *Aralia*, *Triosteum?* *Spermacoce*, *Nabalus*, *Gaultheria*, *Mimulus*, *Phytolacca*, *Sassafras?* *Podostemon* (and others remain to be added),—essentially increasing the remarkable number of Eastern Northern American genera which are represented in an antipodal region, of analogous extreme climate, but not in the less distant regions of Western Europe and Western North America, the greater parts of which are endowed with a more equable climate.

Page 239. In the heading of the third column, second line, occurs a misprint of "and" for "not."

Page 232, line 4, "*Cirsium pumilum*" is to be erased, as it occurs in Missouri, according to Dr. Engelmann.

Salix repens.
Salix herbacea.
Luzula arcuta.
Luzula spicata.
Juncus trifidus.
Carex capitata.
Carex atrata.
Carex rigida.
Phleum alpinum.
Calamagrostis Pickeringii.
Poa laxa.
Aira atropurpurea.
Hierochloa alpina.

They are 37 species in number. Of these all but the five printed in *italic* are natives likewise of Europe.

2. *List of Subalpine Phænogamous Species, which occur mainly in our Alpine Region, but are also found decidedly out of it.*

Alsine Grœnlandica.
Geum radiatum.
Rubus Chamæmorus.
Solidago thyrsoidea.
Solidago Virga-aurea.
Arnica mollis.
Vaccinium uliginosum.
Vaccinium Vitis-Idæa.
Euphrasia officinalis.
Polygonum viviparum.
Empetrum nigrum.
Platanthera obtusata.
Scirpus cæspitosus.
Carex scirpoidea.
Carex capillaris.
Trisetum subspicatum.

Making 16 species; of which all but the five printed in italic are likewise European; and two of these occur in Greenland.

3. *List of Species not found in our Alpine Region, and half of them not even in Subalpine Stations, although they are all Subalpine or Arctic in Europe.*

Saxifraga tricuspidata.
Saxifraga oppositifolia.
Saxifraga aizoides.
Saxifraga Aizoon.
Artemisia borealis.
Juncus Stygius.
Carex gynocrates.

The last two of these seven species are likewise remarkable for not having been found in continental British or Arctic America nor in Labrador; but one of them occurs in Newfoundland, and the other in Greenland.

It would be in order now to consider the range of our species generally north and south. But I will for the present restrict the inquiry to a special and small part of them, namely, to the species which we possess in common with Europe.

The Northward Range in this country of the Phænogamous Species which are common to us and to Europe.

This is an interesting point of inquiry, from its bearings upon the mooted question of the single or multiple origin of the species:—upon whether they may have been diffused each from a common centre, or were originally given to two or more widely separated parts of the world. The arctic regions form one botanical province. The greater part of their plants are com-

mon to the Old and the New Worlds; and the same species is as likely to occur at any two stations within the arctic circle as at any other two stations equally distant. We naturally look northward for the connection of our flora with that of the Old World; and as we meet with United States plants identical with those of Europe, we are interested to know whether they range northward into or near to the area of common northern vegetation. The *data* now in our possession furnish the following results.

Of our species common to Europe, we know only five which do not occur north of the 40th parallel of latitude, or which barely cross this line. These are

Callitriche pedunculata.
Juncus maritimus.
Convallaria majalis.
Cyperus rotundus.
Carex flacca.

The first of these has doubtless been overlooked. The second is a little-known plant with us, and the identification is not perfect. The fourth is a tropical species, and evidently an immigrant into the southern United States as well as into southern Europe, nor is it impossible that our Nut-Grass may again be specifically distinguished from *Cyperus rotundus*. The fifth is here found only in New Jersey, between lat. 40° and 41°. Unless it has been overlooked in the Northern States (which seems unlikely), or unless our plant has been wrongly referred to the variable *Carex flacca*, it affords a remarkable instance of the local occurrence here of a species which is widely diffused in the Old World. It seems not likely to have been introduced from Europe. The third is the most remarkable case; that of the Lily of the Valley (*Convallaria majalis*). This species—or one which I could not in any respect distinguish from it on a comparison of living specimens—abounds in the higher Alleghanies of North Carolina, I believe also in those of Georgia, and it extends north to the Peaks of Otter in Virginia, lat. 37½°, at an altitude of 4000 feet; but it is not known to occur anywhere beyond this; while in Western Europe it extends nearly to lat. 70°. It is not a plant which could well have escaped observation in the Northern States.

The following 15 species are not known to occur north of lat. 45°:

Myosurus minimus.
Subularia aqnatica.
Centunculus minimus.
Veronica officinalis.
Myosotis arvensis.
Salicornia mucronata ?
Polygonum dumetorum.
Castanea vesca, var.
Polygonatum latifolium.
Rhynchospora fusca.
Carex vulpina.
" muricata.
" lævigata.
Spartina juncea.
" stricta.

Myosurus minimus occurs with us only in the valley of the Mississippi,—thence south to Texas and west to the Pacific, but not extending northward beyond lat. 45°. It has all the appearance of being indigenous; and in Oregon it is accompanied by a second species which is also a native of Chili. In Europe it occurs as far north as Finland.

Subularia aquatica seems to be a very rare plant in North America, found only in the northeastern corner of the United States.* From its size, aspect, and place of growth it is exceedingly liable to be overlooked. It is to be sought in Nova Scotia, Newfoundland and Canada East. It reaches lat. 70° in Europe.

Centunculus minimus, which extends northward to lat. 60 in Europe here scarcely passes the 41st parallel in the valley of the Mississippi, where alone it occurs in the Northern States.

Veronica officinalis is certainly indigenous in the Alleghany Mountains south of Pennsylvania, and apparently so in the western part of New York. It is not known north of lat. 44°, and in Europe it does not reach the Arctic circle.

Myosotis arvensis is not common here, and has probably been introduced.

Salicornia mucronata, Bigel., is most probably not identical with its homonym on the coast of Spain.

Polygonum dumetorum (if our P. scandens really belongs to it,) does not pass the 45th parallel with us, while in northern Europe it crosses the Arctic circle.

Our Chestnut is one of the few American trees which can anyhow be identified or confounded with European species. It nowhere occurs north of lat. 44° or 45° in this country; and as the European Chestnut is perhaps not really indigenous in any higher latitude in the Old World, we have here either a very anomalous case in geographical distribution, or else must regard our chestnut as specifically distinct. Analogy would favor the latter view, and (which is more directly to the purpose) so also would some little differences in the fruit, such, however, as would be of small account in case the trees were natives of the same district.

Polygonatum latifolium. This is a case of imperfect identification; the American plant so called being known to us only by specimens sent from Pennsylvania by Muhlenberg to Willdenow.

* In the *Genera of N. American Plants Illustrated*, i, p. 164, no less than in the *Manual*, I have made a mistake in respect to the habitat of this plant, which has kindly been pointed out by Prof. Tuckerman. Nuttall long ago gathered it in the ponds of Paris, Maine. I had supposed that Nuttall's station was rediscovered by Messrs. Tuckerman and Oakes; but I am informed that the locality and the only one now known in this country, is Echo Lake, in the Franconia Notch, New Hampshire, where it was detected in 1844 by Prof. Tuckerman, as is recorded by that accurate botanist, indeed, in the pages of this Journal, for September, 1848.

Rhynchospora fusca, and the three species of *Carex* would certainly be expected to have a more northern range. *C. lævigata* has been found but once. What is called *C. vulpina* is probably not distinct enough from *C. alopecoidea;* and *C. muricata*, if rightly identified, may have been introduced, at least into New England, where it occurs only in suspicious situations, and rarely.

The two species of *Spartina* belong properly to America, being found only in a few places on the coast of Europe, where they seem to have effected a chance lodgment.

The following species, 36 in number, are not known to reach in this country, or at least sensible to pass, the 50th parallel of latitude.

Ranunculus repens.	Atriplex hastata.
Nuphar Kalmiana.	Salsola Kali.
Draba verna.	Humulus Lupulus.
Drosera longifolia.	Betula alba, var.
Sagina procumbens.	Taxus baccata, var.
Oxalis acetosella.	Typha angustifolia.
" stricta.	Vallisneria spiralis.
Geranium Robertianum.	Spiranthes cernua.
Vicia Cracca.	Microstylis monophyllos.
Geum strictum.	Juncus Stygius.
" rivale.	" effusus.
Potentilla argentea.	Lemna gibba.
Lythrum Salicaria.	Najas flexilis.
Circæa Lutetiana.	Zannichellia palustris.
Myriophyllum verticillatum.	Ruppia maritima.
Xanthium strumarium.	Cyperus flavescens.
Samolus Valerandi.	Carex fulva.
Scrophularia nodosa.	Milium effusum.

Upon this list I remark, first, that two of the species, although admitted as indigenous in the *Manual of the Botany of the Northern United States*, were probably introduced from Europe since the settlement of the country; namely *Draba verna* and *Potentilla argentea*. At least the expression of doubt which in the work just mentioned is thrown upon the former, I now think equally applies to the latter. I never saw either of them growing in other than suspicious situations. They are found only in the Eastern United States and in the long-settled parts of Canada; while in Europe the first falls short of, and the second barely enters Lapland.

Lythrum Salicaria has better claims to citizenship, at least in Eastern New England, but it is not clear from suspicion. Douglas gathered it in wet meadows of Upper Canada; but if really indigenous to this country it is surprising that it does not extend farther north.

Some of the forms which I have felt obliged to join with *Xanthium strumarium* seem to be indigenous in the northwestern por-

tion of our district; and so, along the coasts and great rivers, does the variety *echinatum* (which most botanists will still fancy to be very distinct); but the latter, more widely dispersed over the world, is probably an immigrant from farther south. The real home of the species is uncertain: it could not be expected to occur much north of lat. 46°.

Betula alba and *Taxus baccata*, var. *Canadensis* are the only woody plants upon the list. As to the first, I have followed Spach in considering our White Birch identical with the European. It occurs only from the eastern part of Pennsylvania and New Jersey to Maine, and not far from the coast, reaching barely to about the 46th parallel, one or two degrees beyond the Chestnut. In Western Europe *B. alba* extends into the arctic regions. The American tree should be critically compared anew with the European. At present the White Birch and the Chestnut are our only trees here considered as specifically identical with European ones; unless we add our low and procumbent representative of the Yew (*Taxus baccata*, var. *Canadensis*). If a striking difference in habit or mode of growth alone may be relied upon for characters, then our Yew must be specifically distinct from that of Europe. Other differences, however, have not been detected. Our Yew, according to Richardson, merely reaches the southern borders of the Saskatchawan basin, say about lat. 50°. In Oregon there are Yew trees with the port of the ordinary *T. baccata*, which appears not to occur in Northern Asia, although Dr. Hooker recognizes it in the Himalayas.

Drosera longifolia (*intermedia*), *Circæa Lutetiana*, and *Myriophyllum verticillatum* might be expected to extend farther north than lat. 47°, but they do not occur in Lapland.

Juncus Stygius is the most remarkable member of this list. Its only known habitat on the North American *continent* is a bog adjoining a small lake in Jefferson county, New York, lat. 44°, where I found it in great abundance twenty-four years ago. But it has been collected in Newfoundland, probably south of lat. 49°. This is a mainly Scandinavian species, of high northern range, not found in Denmark, according to Fries, but extending to Bavaria and the Alps. It has been said to occur in Northeastern Siberia, but Trautvetter corrects this in his *Florula Ochotensis:* what he had taken for it, he finds on reëxamination, to be *J. castaneus*. In all probability it grows in Lower Canada: but if in Northern British America it could hardly have been overlooked by Richardson and Drummond.

Carex fulva is another species unknown in Northern British America, found at only one station in the United States, and elsewhere only in Newfoundland, whence Goodenough received the specimens on which he founded the species. In Europe, it is scarcely found north of lat. 60.

Spiranthes cernua, if really European, is found only on the west coast of Ireland, and belongs to the same remarkable category as *Eriocaulon septangulare*, of the next list.

For the rest of the list, no particular remarks are needed. Their northern range in this country corresponds generally with that in Europe, making allowance for the difference in climate; that is, they range ten or fifteen degrees farther north in Western Europe than in North America. This is true even of *Vallisneria spiralis*, a plant of temperate climates, with a wide southern range, and neither Scandinavian nor North German. In North America it extends to about lat. 46° : in Russia it is recorded as occurring near St. Petersburg, lat. 60°.

The following species, 56 in number, range north of lat. 50, and many of them have been traced up to lat. 55°, but not much if any beyond the latter parallel.

Anemone nemorosa.
Cerastium arvense.
Trifolium repens.
Ludwigia palustris.
Sium angustifolium.
Bidens cernua.
Gnaphalium uliginosum.
Chimaphila umbellata.
Monotropa Hypopitys.
Statice Limonium.
Utricularia vulgaris.
" minor.
Lycopus Europæus.
Myosotis palustris.
Calysegia sepium.
Salicornia herbacea.
Rumex maritimus.
Ceratophyllum demersum.
Acorus Calamus.
Sparganium ramosum ?
" angustifolium.
Lemna trisulca.
" polyrhiza.
Potamogeton pectinatus.
" prælongus.
" lucens.
" heterophyllus.
Scheuchzeria palustris.
Alisma Plantago.
Anacharis Canadensis.
Liparis Lœselii.
Platanthera bracteata.
Eriocaulon septangulare.
Eleocharis acicularis.
Scirpus maritimus.
Eriophorum alpinum.
Carex teretiuscula.
" tenella.
" tenuiflora.
" maritima.
" irrigua.
" pallescens.
" flava.
" filiformis.
" Pseudo-Cyperus.
" ampullacea.
Leersia oryzoides.
Calamagrostis arenaria.
Kœleria cristata.
Glyceria aquatica.
" fluitans.
Poa serotina.
" compressa.
Phragmites communis.
Triticum caninum.
Aira flexuosa.

Concerning two plants on this list, doubts may fairly be raised whether they are indigenous to this continent, viz. *Gnaphalium uliginosum* and *Poa compressa*. The former is one of our commonest plants, but is only found along damp road-sides and in ground which is or has been tilled. It is found on the Saskatchawan ;

but if there restricted to similar situations, I should consider it one of the species unwittingly introduced by man from Europe. *Poa compressa* here has wholly the appearance of a naturalized plant. Richardson and Drummond gathered it also on the Saskatchawan, I know not in what stations. E. Meyer records it as in Labrador.

Triticum caninum, like the common Couch-Grass (*T. repens*), as it generally occurs with us, is evidently of European derivation; but both species are indigenous from our northern borders northward and westward.

The White Clover (*Trifolium repens*) which springs up so copiously and promptly wherever forests are destroyed and the land turned into pasture, is in the same category, being wild at the north and the far West, and undoubtedly imported likewise at the settlement of the country.

Of *Cerastium arvense*, and probably of *Acorus Calamus* also, we have within our limits both an indigenous and an introduced stock.

With *Anacharis Canadensis* it is not certain that the German and Russian plant is identical, the flowers being unknown there: nor, if so, are we sure that the plant is truly indigenous on the continent of Europe any more than in England, although it is very likely to be so.

Platanthera bracteata is placed upon the list, although with doubt, Dr. Lindley and Sir Wm. Hooker having expressed the opinion that it is identical with *P. viridis* of Europe.

Eriophorum alpinum, a Scandinavian plant of high range,—also found on mountains in Western Europe as far south as the Alps, but scarcely extending into Siberia,—is not rare with us in cold peat bogs, from Pennsylvania to Lake Superior and Maine. It is also a native of Newfoundland, although not mentioned as such by Hooker. I think I have seen specimens from Lower Canada. But no more northern habitats are known except Michaux's, i. e. Lake Mistassins and Hudson's Bay, say lat. 51°. It surely ought to grow in Labrador; but it is nowhere recorded from that region, nor from Greenland.

Eriocaulon septangulare, an Eastern North American plant not ranging beyond lat. 55°, but singularly reappearing only in a few stations in the nearest part of Europe, requires some notice in a different connexion.

It is worth remarking that it is in the interior of the continent, namely, in the Saskatchawan country (long. 95° to 105°,) and not along the coast, that most of the plants of this list attain their highest latitude. We should expect this, as regards the eastern coast, from the rise of the isothermal lines and of the limit of trees on passing westward, and from the great rise of the isotheral lines in the district referred to. Still it will appear singular that only three of these 56 species are recorded as natives of Labrador,

(namely, *Poa compressa*, already mentioned, *Statice Limonium*, and *Chimaphila umbellata*), until it is noted that the principal Labrador collections known were made between lat. 56° and 58°. Many Newfoundland and Canadian plants doubtless inhabit southern Labrador: but I can add only one of these on the present list, namely, *Calamagrostis arenaria*. Only about a dozen of these 57 species appear to occur north of the 50th parallel on the Pacific coast: but this number includes much the larger part of the species on this list which extend westward to the Pacific at all, even in a lower latitude.

The following 42 species range north of lat. 55°, but do not, as far as we know, cross the 60th parallel:

† Hepatica triloba.
†* Coptis trifolia.
Spiræa salicifolia.
† " Aruncus.
Agrimonia Eupatoria.
† Geum macrophyllum.
† Circæa alpina.
* Saxifraga Aizoon.
† Galium trifidum.
† Galium Aparine.
† " triflorum.
†* Plantago maritima.
† Glaux maritima.
Limosella aquatica.
† Veronica Anagallis.
† " serpyllifolia.
† Brunella vulgaris.
Calla palustris.
Lemna minor.
Potamogeton pusillus.
" compressus.
Potamogeton perfoliatus.
† " natans.
†* Triglochin maritimum.
†* Listera cordata.
†* Streptopus amplexifolius.
Scirpus lacustris.
† " sylvaticus.
† Rhynchospora alba.
† Carex pauciflora.
† " * canescens.
† " stellulata.
† " limosa.
† " Buxbaumii.
" * panicea.
" Œderi.
† Agrostis canina.
" vulgaris.
† Cinna arundinacea.
† Glyceria maritima.
† " distans.
† Poa annua.

Only 8 of these 42 species are recorded as natives of Labrador; namely, those marked with an asterisk in the list; while 29 of them are in Bongard's Florula of the Island of Sitcha (lat. 57°–58°) on the North West coast. These are marked with a (†). Among them are six of the eight Labrador species, namely, all except *Saxifraga Aizoon* and *Carex panicea*.

Two species only of the above list are doubtful natives of the United States; viz. *Galium Aparine*, which here is not found beyond Canada, and only in doubtful situations, but it seems to be truly indigenous on the Northwest coast; and *Poa annua*, a cosmopolite plant, found all round the world in high latitudes. *Agrostis vulgaris* and *A. canina* are in the same category with *Triticum caninum* and *T. repens*,—represented at the north by an indigenous, but generally by an imported stock.

Spiræa Aruncus claims a place in the list of disjoined species. It occurs in the Catskill mountains, New York, and southward along the whole extent of the Alleghanies; but here it ranges no farther northward. It is not a Scandinavian plant; but from France it extends eastward through Northern Asia to Kamtschatka, and thence to Sitcha and the mouth of the Oregon.

The following 63 species cross the 60th parallel, but so far as I can ascertain, are not known to cross the arctic circle on this continent.

* Caltha palustris.
Actæa spicata (rubra).
Turritis glabra.
Draba memorosa.
†Stellaria longifolia.
†Spergularia rubra.
Lathyrus palustris.
Fragaria vesca.
* Epilobium palustre.
†* Ligusticum Scoticum.
†* Linnæa borealis.
* Lonicera cærulea.
Galium boreale.
†Achillea Millefolium.
Artemisia Canadensis.
Lobelia Dortmanna.
* Campanula rotundifolia.
†* Ledum latifolium.
* Pyrola chlorantha.
†* Moneses uniflora.
* Primula farinosa.
Naumburgia thyrsiflora.
Utricularia intermedia.
†* Pinguicula vulgaris.
Veronica scutellata.
* Rhinanthus Crista Galli.
Scutellaria galericulata.
Stachys palustris.
†* Menyanthes trifoliata.
Blitum capitatum.
Polygonum amphibium.
†* " aviculare.
Callitriche verna.
" autumnalis.
†* Myrica Gale.
* Alnus incana.
Typha latifolia.
Sparganium simplex.
" natans.
* Triglochin palustre.
Goodyera repens.
Calypso borealis.
Corallorhiza innata.
Smilacina stellata.
†* " bifolia.
* Allium Schœnoprasum.
Juncus filiformis.
" Balticus.
" articulatus.
" bufonius.
Eleocharis palustris.
Scirpus pungens.
†Eriophorum vaginatum.
Carex chordorrhiza.
" aquatilis.
" salina.
† " livida.
† " vesicaria.
Alopecurus aristulatus.
Poa nemoralis.
* " pratensis.
Aira cæspitosa.
Phalaris arundinacea.

Twenty of these marked (*) are in the flora of Labrador; fifteen, marked (†) in that of Sitcha; and nine are common to the two.

It would appear, therefore, that of our Phænogamous plants common to Europe, only

5 do not range north of lat. 40°.
20, or 6 per cent, do not range north of lat. 45°.
56, or 17½ per cent, do not range north of lat. 50°.

113, or 35 per cent, do not pass north of lat. 55°.
155, or 48½ per cent, do not pass north of lat. 60°.
218, or 68 per cent, scarcely, if at all, cross the Arctic circle.

In this inquiry we have thus far left our alpine and even subalpine species, common to Europe, wholly out of view, as not properly belonging to our temperate flora, and as expected to extend northward beyond the Arctic circle. In a few cases, however, this expectation is not exactly borne out. For instance, *Viola palustris* is found only in the alpine region of the White Mountains, in Labrador, and perhaps in the Rocky Mountains about lat. 42°, but has not been noticed in arctic America proper. It occurs, however, in Greenland as well as in Kamtschatka.

Potentilla frigida,* or the plant of the White Mountains which I take for it, has not been elsewhere found in this country (unless confounded with some other species) except in Greenland, between lat. 73° and 80°, by Dr. Kane; nor is it known in the north of Europe!

Sibbaldia procumbens, although found in Labrador and Greenland on one side, and on the northern Rocky Mountains and at Unalaschka on the other, has not been detected in Arctic America within the Arctic circle.

Gnaphalium supinum, a rare plant of the White Mountains, has been elsewhere detected upon the continent only at Labrador: and it also occurs in Greenland.

Cassiope hypnoides, found on all our alpine summits, elsewhere occurs only in Labrador and Greenland, on the one hand, and at Unalaschka, below the Arctic circle, on the other.

Phyllodoce taxifolia occurs only on the White Mountains, in Labrador, and in Greenland.

Veronica alpina, although approaching the Arctic circle both east and west, is not recorded as crossing it, though it probably does so.

Salix phylicifolia occurs only on the White Mountains and in Labrador.

Juncus trifidus, an abundant plant in our alpine districts, is not recorded from any other part of North America, excepting Newfoundland!

Carex capitata, although found on Hudson's Bay and on the Rocky Mountains, is not recorded from within the Arctic circle.

Phleum alpinum, found in Labrador, Greenland, and Unalaschka, is not recorded from within the Arctic circle. On the other hand *Aira atropurpurea*, a Lapland species found on the

* The habitat of this rare plant has been casually left out of the Manual of Botany of N. U. States, p. 118. It was discovered, out of flower, by Dr. Robbins (who has long been one of the most zealous and successful explorers of New England botany), at a single station in the alpine region of the White Mountains of New Hampshire, where it is still rather abundant. Later the indefatigable Prof. Tuckerman detected it on Mount LaFayette, of the Franconia range. These are the only stations known on this continent.

White Mountains, is remarkable for not being found in Labrador, nor in Greenland that I am aware of, while it occurs on the Northwest Coast below lat. 60°, but nowhere in Siberia, Kamtschatka, &c., nor was it known in Arctic America until lately collected between Point Barrow and Mackenzie river, by Capt. Pullen, according to Mr. Seemann.

As to our subalpine list: *Alsine Grœnlandica* is wrongly said in the Manual to be European, as it has not been found beyond Greenland. It also occurs in Labrador, and with us as far south as the low Shawangunk Mountains in the southern part of New York; but is entirely unknown in Canada and in Arctic America.

Carex gynocrates, which we should expect to be alpine and arctic, but which is not known as either in this country, is connected with Lapland by the intermediate station of Greenland. Excepting this and *Juncus stygius* (which has already been commented on), and perhaps also *Euphrasia officinalis*, all our strictly subalpine species, as well those enumerated as to have been expected to be so, which are common to us and to Europe, extend northward along the central region of the continent quite to the Arctic sea-coast. While, curiously enough, eleven, or one-third of our strictly alpine species common to Europe,—all but one of them arctic in the Old World,—are not known to cross the arctic circle on this continent. This, however, might perhaps have been expected, as it seems almost certain that the interchange of alpine species between us and Europe must have taken place in the direction of Newfoundland, Labrador and Greenland, rather than through the polar regions; and this a critical study of the distribution of our plants northward would be likely to show.

Adding accordingly a dozen alpine or subalpine species, we have about 230 Phænogamous species common to Europe, or 72 per cent, which have not been detected within the Arctic circle upon the American continent.

Our species common to Europe which do extend into the Arctic zone,—exclusive of all those enumerated in the three lists of alpine, subalpine, and the seven *should-be* alpine or subalpine species given on p. 63, are these (52 in number):

Dicotyledoneæ.

Ranunculus aquatilis, var.
" flammula, var.
" sceleratus,
Nasturtium palustre.
Cardamine pratensis.
" hirsuta.
Arabis hirsuta.
Barbarea vulgaris.
Erysimum cheiranthoides.
Drosera rotundifolia.
Parnassia palustris.
Honkenya peploides.
Mœhringia lateriflora.
Stellaria longipes.
" uliginosa.

Stellaria crassifolia.
" borealis.
Sagina nodosa.
Lathyrus maritimus.
Potentilla Norwegica.
" Anserina.
" fruticosa.
" palustris.
Epilobium angustifolium.
Myriophyllum spicatum.
Hippuris vulgaris.
Ribes rubrum.
Viburnum Opulus.
Artemisia Canadensis.
Taraxacum Dens-leonis.
Vaccinium Oxycoccus.
Arctostaphylos Uva-Ursi.
Cassandra calyculata.
Andromeda polifolia.
Pyrola rotundifolia.
Pyrola minor.
Primula Mistassinica.
Mertensia maritima.
Gentiana detonsa.
Chenopodina maritima (?)
Alnus viridis.
Juniperus communis.

Monocotyledoneæ.

Luzula pilosa.
" parviflora.
" campestris.
Juncus bulbosus.
Eriophorum polystachyon.
" gracile.
Carex vulgaris.
Festuca ovina.
Triticum repens.
Hierochloa borealis.

If any interesting relation is to be traced between the more or less boreal range of our temperate species common to Europe, and the natural classes or orders they severally belong to, the means of instituting the comparison are at hand in the various foregoing lists. The subjoined columns give a comparison in this respect between our 52 non-alpine plants which extend into the Arctic zone, and the almost equal number whose northern limit is between the 40th and the 50th parallels. It shows nothing, however, except the diminution of the number of the orders, and of non-glumaceous Endogens, in high latitudes.

Non-alpine species of the above list with their boreal limit with the Arctic circle.		Non-alpine species with their boreal limit between lat. 40° and 50°.	
Ranunculaceæ,	3	Ranunculaceæ,	2
Cruciferæ,	6	*Nymphæaceæ*,	1
Droseraceæ,	1	Cruciferæ,	2
Parnassiaceæ,	1	Droseraceæ,	1
Caryophyllaceæ,	7	Caryophyllaceæ,	1
Leguminosæ,	1	*Oxalidaceæ*,	2
Rosaceæ,	4	*Geraniaceæ*,	1
Onagraceæ,	3	Leguminosæ,	1
Grossulaceæ,	1	Rosaceæ,	3
Caprifoliaceæ,	1	*Lythraceæ*,	1
Compositæ,	2	Onagraceæ,	2
Ericaceæ,	6	Compositæ,	1
Primulaceæ,	1	Primulaceæ,	2
Borraginaceæ,	1	*Scrophulariaceæ*,	2
Gentianaceæ,	1	Borraginaceæ,	1
Chenopodiaceæ,	1	Chenopodiaceæ,	3

Betulaceæ,	1
Coniferæ,	1
Dicotyledoneæ,	42
Juncaceæ,	4
Cyperaceæ,	3
Gramineæ,	3
Monocotyledoneæ,	10
21 orders,	52 species.

Polygonaceæ,	1
Urticaceæ,	1
Cupuliferæ,	1
Betulaceæ,	1
Coniferæ,	1
Dicotyledoneæ,	31
Typhaceæ,	1
Lemnaceæ,	1
Naiadaceæ,	3
Hydrocharidaceæ,	1
Orchidaceæ,	2
Liliaceæ,	1
Juncaceæ,	2
Cyperaceæ,	6
Gramineæ,	3
Monocotyledoneæ,	20
30 orders,	51 species.

Considered as to size and duration of the plants in connection with geographical range, our 320 species common to Europe, are divided as follows:

Only 3 of them are trees, namely, the Chestnut, White Birch, and Yew; and the latter is no tree in this country. All three have been and generally are still taken for peculiar American species, perhaps correctly. None of them extend north quite to lat. 50°, the Chestnut not beyond 45°. Two of them range a little south of lat. 40°, and one, the Chestnut, considerably south of lat. 36°. Their geographical distribution, taken in connexion with the comparatively restricted area of trees, favors the suspicion that these are specifically different from the European species.

Only 15 species are shrubs. All of them occur as far north as lat. 60°, or else are alpine, and 10 grow within the arctic circle.

Then 12 are suffruticose or suffrutescent plants, all of them arctic-alpine or subalpine, and with their southern limit under lat. 40°, except two; namely, *Arctostaphylos Uva-ursi*, which ranges from the arctic shores to lat. 35° and across the whole breadth of the continent at its widest part; and *Chimaphila umbellata*, which, from its northern limit of about 55°, is equally broadly distributed over the continent, and extends southwards even into Mexico.

The remaining 290 species are all herbs; and about 260 of them are perennials. Of the 30 annuals and biennials, few have a high boreal range, but at least 20 of them are among our species of widest southern range.

The Southward Range in this Country of our Phænogamous Species common to Europe.

This is not a subject of so much interest as the northern range. The subjoined table exhibits the main facts of the case, as well as I can now determine them, as respects our species which are neither alpine nor subalpine.

No. of species.	Whose boreal limit is	Of these the southern limit is beyond			Found on DeCandolle's list of species of *vast area*; out of the whole No. (column 1.)	out of those rang'g S. of 40° (columns 3, 4, 5).
		Lat. 30°.	Lat. 36° 30'.	Lat. 40°.		
52	+66° 30'	6 spec.	11 spec.	16 spec.	10 spec.	9 species.
63	+60°–66°	9 "	16 "	8 "	11 "	9 "
42	+55°–60°	16 "	9 "	7 "	10 "	10 "
57	+50°–55°	17 "	12 "	16 "	10 "	10 "
36	+45°–50°	10 "	10 "	8 "	7 "	7 "
15	+40°–45°	4 "	3 "	4 "	1 "	1 "
5	+40°	2 "	2 "	1 "	0 "	0 "

DeCandolle's list (in Geogr. Bot. p. 564, et seq.) of Phænogamous species of vast area comprises those which, in their actual dispersion, are estimated to be diffused over at least one-third of the terrestrial surface of the globe. It therefore rarely includes maritime plants, although these are so wide-spread. Nor does it include several of our species which as wild plants have better claim than some which are admitted. Indeed a large proportion of those common to Europe which range south of lat. 36° 30′ are very widely spread species.

For example, the six *amphigæan* species which range from within the arctic circle to south of the 30th parallel, or to the Gulf of Mexico, are *Ranunculus aquatilis*, *Nasturtium palustre*, *Cardamine hirsuta*, *Drosera rotundifolia*, *Taraxacum Dens-leonis*, (probably introduced southward) and *Luzula campestris*, all but one nearly cosmopolite, and that one (*Drosera*) diffused over most northern temperate regions.

And the 9 which range from near the arctic circle to the same low latitude are *Spergularia rubra*, *Polygonum amphibium* and *aviculare*, *Callitriche verna* and *autumnalis*, *Typha latifolia*, *Juncus bufonius*, *Eleocharis palustris*, and *Scirpus pungens*. (*Caltha palustris*, which is on DeCandolle's list, does not extend quite so far south.) And among the 16 which range from lat. 55°–60° down to lat. 30° are such plants as *Agrimonia Eupatoria*, *Veronica Anagallis* and *serpyllifolia*, *Limosella subulata*, *Brunella vulgaris*, *Lemna minor*, *Potamogeton perfoliatus* and *natans*, *Scirpus lacustris*, and *Poa annua*,—a few of them not really indigenous there. One hardly to be expected is *Hepatica triloba*, which occurs in Florida.

Of our alpine species common to Europe, of course none occur south of Northern New York. And as to the eleven on the sub-

alpine list, only two have been observed anywhere in the Alleghanies, (and these only south of lat. 36° 30′), viz. *Scirpus cæspitosus* and a form of *Trisetum subspicatum*. None of the rest are known in the Eastern United States so far south as lat. 40°.

The re-appearance of some northern species in high southern latitudes is another matter, and to be considered under the head of *disjoined species*.

The range of our amphigæan species might be considered in reference to the stations they affect or the medium in which they grow; but this would be likely to bring out no important results beyond the familiar ones that aquatic, palustrine and maritime species are among those of widest range in latitude. The lists already given enable any botanist to do it.

The Species common to this Country and to Europe in respect to the size of the Orders and Genera they belong to.

The species which we possess in common with Europe, being generally speaking those of widest geographical range, form good materials ready to our hands, as far as they go, for the enquiry whether there is any assignable relation between the size of a natural group and the area occupied by the species;—a subject which DeCandolle has briefly discussed, as regards families. By gathering the *data* from the table which begins on p. 208, it will be seen that our nine largest families (as enumerated in order on p. 213) come a little nearer to comprising half our amphigæan species than they do to half our whole number of species: i. e., they contain 158 species, or two less than half. But the tenth family, *Labiatæ*, being very poor in these common species, brings up the number to only 162, or barely two above half of the 320. And our different large families present such marked differences in the ratio of their amphigæan species,* that it is clear no results of the least moment are to be obtained in this way.

To be of any value, at least upon our limited scale, the comparison should be made with genera, as Mr. Darwin suggests; and from some investigations of his own, this sagacious naturalist inclines to think that species in large genera range over a wider area than the species of small genera do. Our 320 amphigæan species evidently tend to confirm this view. They belong to 171

* Compositæ,	out of	273	species,	9	are common to Europe.
Cyperaceæ,	"	213	"	48	" "
Gramineæ,	"	162	"	32	" "
Leguminosæ,	"	91	"	4	" "
Rosaceæ,	"	71	"	16	" "
Ericaceæ,	"	62	"	19	" "
Scrophulariaceæ,	"	54	"	10	" "
Orchidaceæ,	"	51	"	10	" "
Ranunculaceæ,	"	49	"	10	" "
Labiatæ,	"	49	"	4	" "

genera. Of these only 18, or 10·5 per cent, are monotypic, while 13·3 per cent of our whole number of genera are monotypic: 23 genera or almost 13 per cent contain only two or at most three good species apiece, and about as many more have only four or five good species. Therefore 64 of the smaller genera, or 37 per cent, fully come up to the general average of species to genus in our Phænogamous plants generally, viz. three each (supra, p. 216); while on the other hand, 15, or 8 per cent, are very large genera, such as *Carex*, *Solidago*, *Cyperus*, *Salix*, *Allium*, *Galium*, *Trifolium*, *Gentiana*, *Ranunculus*, &c. Though many of these are not very large genera in our region, nor do those that are large particularly abound in amphigæan species.

Comparison of the Flora of the Northern United States with that of Europe in respect to the Similar or Related Species.

Two floras may be, perhaps, as nearly related through their allied as through their identical species: at any rate, the comparison in this respect is equally important to be made. Such comparisons, however, are much more difficult, owing to the impossibility of estimating the degrees of resemblance among species, or at least of expressing them in any precise or definite way, or of bringing shades of difference to any common standard. In theory, indeed, only one grade of resemblance is supposed to be expressed in genera. But genera,—even those whose circumscription is either clearly defined in nature (which is far from being always the case) or is generally agreed upon, are by no means groups of equal value throughout; and the species of every genus, when several or numerous, resemble each other in very unequal degrees.

Still no two analogous but geographically separated floras of any size are so well known, as to their Phænogamia, and afford generally such facilities for the comparison of their related species, as those of the Northern United States and of Northern Europe.

If we judge of their relationship from the large proportion of the genera common to the two, we might infer it to be very close. After correcting a little the numbers published in the former article, on p. 216 *et seq.*, we count 326, or not much less than one-half of the 681 genera as belonging also to Europe. This indicates a great *amount* of related vegetation in the two floras, no doubt; but of the *degree* of relationship, taken comparatively, it gives us no correct idea, until we know how many of the genera common to the two are almost cosmopolite, or are wide-spread over the cooler parts of nearly the whole northern hemisphere; and how many are peculiar or strictly characteristic. Now, on going over the list, I find that an extraordinarily large proportion of the genera common to our flora and to Europe belong also

to the floras of almost all the temperate regions of the world. Out of the 326 genera,

284, or 87 per cent are diffused around the northern hemisphere, or over the greater part of it;

201, or 61 per cent, extend into the tropics or cross the equator.

177, or considerably over half, are both widely diffused over the northern temperate zone and extend into or beyond the tropics:—leaving

18, or only 5½ per cent, as nearly peculiar to Eastern North America and to Europe; and very few even of these are strictly peculiar.

A simple enumeration of them will show how trivial a part these 18 American-European genera play in the two floras. They are:

Bellis,	Fedia,
Cakile.	Hottonia,
Calla,	Liparis,
Carpinus,	Melampyrum,
Cassandra,	Narthecium,
Cercis,	Ostrya,
Convallaria,	Scheuchzeria,
Corema,	Subularia,
Dupontia.	Waldsteinia.

Most of these are very small genera, four if not five of them having only a single species each. *Cercis* actually extends to California, and south to near the tropical line. *Dupontia* is a purely arctic genus, to which our species, so called, is referred with much misgiving. *Hottonia* is said to have a third species in Java, which would exclude it from the present list. Moreover, five of these genera are represented only by identical species.

The special resemblance of our flora to that of Europe, it is clear, is not owing simply either to the large proportion of genera in common, or to any thing striking or important in the few genera nearly or quite peculiar to the two. The latter, indeed, are insignificant in our flora, and not to be compared, as to any features they impart, with the much more numerous and really characteristic genera which are shared by the Eastern United States and Eastern temperate Asia. We must look for it in the species, partly in the identical ones (already noticed), and partly in those which closely answer to each other in the two floras.

The citation of *representative species*, to be of much value, should be more critical than it generally is in such comparisons. The degrees of affinity should be classified as strictly as the subject admits of, under several heads; beginning with the plants so closely related in each that they form a sort of *limbo* between

the regions of identical and of allied species on either side, and which may or may not be reckoned specifically the same, according to our varying knowledge, and according to the views which different authors take respecting species. A good analysis of the subjects of comparison might perhaps be made into,

1. *Geographical Varieties*, or those cases in which the American plant is always or generally distinguishable from the European in some point or other, and is not unlikely to be reckoned specifically different even by sound botanists.

2. *Very Close Representative Species*, admitted as distinct, but not unlikely many of them to be reduced to geographical varieties.

3. *Strictly Representative Species*, pretty exactly answering to each other in the two floras, but of which there can be little if any question of specific identity.

4. *Strictly Congeneric Species*, but not falling into either of the former categories.

5. *Divergent Congeneric Species*, where the American type belongs to a different section or subgenus from the European.

To which these might be added, 6th, species of strictly analogous or representative genera.

I must not attempt here anything beyond an enumeration, made *currente calamo*, of such examples in question as occur to me under the first three heads:

1. *American Geographical Varieties, which not only have been, but are not unlikely to be again distinguished as Species, and therefore to be merged in No.* 2.

Ranunculus repens, Amer.:	formerly R. Marilandicus, &c.
Actæa spicata, vars. alba and rubra:	A. alba and rubra.
Nasturtium palustre, fructu brevi:	N. hispidum, &c.
Ribes rubrum, Amer.:	R. albinervium.
Solidago virgaurea, Amer., vars.:	S. humilis and multiradiata.
Monotropa Hypopitys, Amer.:	M. lanuginosa.
Statice Limonium, var.:	S. Caroliniana.
Samolus Valerandi, var. Amer.:	S. floribundus.
Lycopus Europæus, var. sinuatus:	L. sinuatus.
Stachys palustris, vars:	S. aspera, glabra, cordata, &c.
Myosotis palustris, var. laxa:	M. laxa.
Castanea vesca, var. Americana:	C. Americana.
Betula alba, var. populifolia:	B. populifolia.
Taxus baccata, var. Canadensis:	T. Canadensis.
Trisetum subspicatum, var. molle:	T. molle.

2. *Very Close Representative Species, almost all of them more or less liable to be reduced to Geographical Varieties.*

Northern United States.	*Northern Europe.*
Pulsatilla Nutalliana,	P. vulgaris.
Delphinium exaltatum,	D. elatum.
Berberis Canadensis,	B. vulgaris.
Nymphæa odorata,	N. alba.
Nuphar Kalmiana,	N. lutea.
Arabis lyrata,	A. petræa.
Sisymbrium canescens,	S. Sophia.
Cakile Americana,	C. maritima.
Viola Muhlenbergii,	V. canina.
Elatine Americana,	E. triandra.
Geranium Carolinianum,	G. dissectum.
Impatiens pallida and fulva,	I. noli-tangere.
Geum album,	G. urbanum.
Potentilla paradoxa,	P. supina.
Rubus strigosus,	R. Idæus.
Amelanchier Canadensis,	A. vulgaris.
Epilobium coloratum,	E. tetragonum.
Tillæa simplex,	T. aquatica.
Sedum telephioides,	S. Telephium.
Chrysosplenium Americanum,	C. oppositifolium.
Cornus Canadensis,	C. Suecica.
Sambucus pubens,	S. racemosa.
Galium trifidum.	G. palustre.
Valeriana sylvatica,	V. dioica.
Nardosmia palmata,	N. frigida.
Antennaria plantaginifolia,	A. dioica.
Ledum latifolium,	L. palustre.
Veronica Americana,	V. Beccabunga.
Melampyrum Americanum,	M. pratense.
Lithospermum latifolium,	L. officinale.
Euphorbia obtusata,	E. platyphylla.
" commutata,	E. Peplus.
Fagus ferruginea,	F. sylvatica.
Juniperus Virginiana,	J. Sabina.
Allium Canadense,	A. vineale.
Veratrum viride,	V. album.
Narthecium Americanum,	N. ossifragum.
Eleocharis rostellata,	E. multicaulis.
Scirpus pungens, &c.,	S. triqueter.
Hordeum pusillum,	H. maritimum.
Elymus striatus,	E. Europæus.
" mollis,	" arenarius.

3. *Strictly Representative Species, probably few of them to be confounded.*

Northern United States.	*Europe.*
Atragene Americana,	A. alpina.
Clematis ochroleuca,	C. integrifolia.
Ranunculus alismæfolius,	R. Lingua.
" abortivus,	R. auricomus.
Isopyrum biternatum,	I. thalictroides.
Aconitum reclinatum,	A. Lycoctonum.
Nasturtium lacustre,	N. amphibium.
Draba arabisans,	D. incana.
Lepidium intermedium,	L. ruderale.
Senebiera didyma,	S. Coronopus.
Tilia Americana,	T. Europæa,
" heterophylla,	T. argentea.
Staphylea trifolia,	S. pinnata.
Acer spicatum,	A. Tartaricum.
" saccharinum,	A. platanoides.
Hedysarum boreale,	H. obscurum.
Cercis Canadensis.	C. Siliquastrum.
Prunus Virginiana,	P. Padus.
Rubus triflorus,	R. saxatilis.
Pyrus (Sorbus) Americana,	P. aucuparia.
Ribes floridum,	R. nigrum.
Saxifraga Virginiensis,	S. nivalis.
" Pennsylvanica.	S. hieracifolia.
Hydrocotyle interrupta,	H. vulgaris.
Sium lineare,	S. latifolium.
Cornus sericea and stolonifera,	C. sanguinea.
Lonicera grata,	L. Caprifolium.
Sambucus Canadensis,	S. nigra.
Viburnum lantanoides,	V. Lantana.
Fedia radiata,	F. olitoria, &c.
Aster flexuosus,	A. Trifolium.
Bidens connata,	B. tripartita.
" chrysanthemoides,	B. cernua.
Artemisia Ludoviciana,	A. vulgaris.
Gnaphalium purpureum,	G. sylvaticum.
Hieracium Canadense,	H. prenanthoides, &c.
Azalea calendulacea,	A. Pontica.
Ilex opaca,	I. Aquifolium.
Diospyros Virginiana,	D. Lotus.
Androsace occidentalis,	A. elongata.
Trientalis Americana,	T. Europæa.
Hottonia inflata,	H. palustris.
Mentha Canadensis,	M. arvensis.
Myosotis verna,	M. stricta.
Gentiana Saponaria,	G. Pneumonanthe.
Asarum Canadense,	A. Europæum.
Corema Conradii,	C. alba.
Ulmus Americana,	U. pedunculata.

Ulmus fulva,	U. montana.
Celtis occidentalis,	C. australis.
Morus rubra,	M. nigra.
Urtica gracilis,	U. dioica.
Parietaria Pennsylvanica,	P. officinalis.
Platanus occidentalis,	P. orientalis.
Corylus Americana,	C. Avellana.
Carpinus Americana,	C. Betulus.
Ostrya Virginica,	O. vulgaris.
Alnus serrulata,	A. glutinosa.
Salix lucida, &c.,	S. pentandra, &c.
Populus tremuloides,	P. tremula.
Abies balsamea,	A. pectinata.
" nigra and alba,	" excelsa.
Larix Americana,	L. Europæa.
Sagittaria variabilis,	S. sagittifolia.
Cypripedium pubescens,	C. Calceolus.
Smilax rotundifolia, &c.,	S. aspera.
Polygonatum biflorum,	P. multiflorum.
" giganteum,	" officinale.
Erythronium Americanum and albidum,	E. dens-canis.

I omit, for the most part, the large genera, in which it becomes a nice question rightly to pair off representative species.

In all these lists it is sometimes the case that the species or forms of the second column also are indigenous to the United States, or to North America.

Adding now our about 115 closely representative species (of the second and third lists) to the 320 identical ones, we have a total of 435, or over one-fifth of our Phænogamous species, as the same as, or very much like European plants; and enough more of good representative forms might be selected from the large genera (Carex, Salix, Quercus, Juncus, &c.) to bring the proportion up to nearly one-third.

Equally prominent European features of our flora might be traced in the fourth list, if filled out. Here the greater number of allied species would fully make up for the somewhat less close affinity, and so exhibit an equal amount of resemblance. And this brings me to remark that,

Finally, it is in the number of familiar European forms,—especially of those most striking to the eye and most effective in the landscape,—that the general likeness of the vegetation, and the preponderant share of the botanical affinity of our flora to that of northern Europe consists. This might be illustrated in a variety of ways.

A very large part of the more conspicuous and popularly well-known European genera are represented here;—if not in indigenous, at least in naturalized plants, which the common observer never thinks of eliminating. Illustrations of so familiar a fact are superfluous.

Of trees and shrubs,—the most conspicuous members of a flora, and many of them among the most abundant in individuals,—I find only eleven genera in the British flora which are not in ours likewise; and five of these are probably not truly indigenous to Great Britain. Of the remainder we have here genera strictly analogous to each, except to *Erica*, *Daphne*, and *Ulex*. On the other hand, indeed, we have 46 extra-European genera of trees and shrubs, showing our superior richness in this respect, which has often been remarked upon: but, excepting Heaths, Furze, and Tamarisks, we lack scarcely any North European arborescent or woody type.

As to glumaceous plants,—likewise so prolific in individuals,—only three British genera of *Cyperaceæ* and 9 of Grasses are wanting here.

A vast preponderance of our species throughout belong to genera common to Europe. This has already been noted, as respects the orders, in my former article (p. 216). It is equally true as to the genera, as the following data serve to show.

The Phænogamous genera in our flora, as has been already stated, average three species apiece; and fully half of them are represented by more than one species. But of the 353 extra-European genera, as many as 234, or 66 per cent are represented in our flora by only one species; and of the remaining 34 per cent only 34 genera exceed the general average of 3 species. Only eleven of these, I believe, have as many as 9 species, and six of them have from 10 to 18, which is the maximum. On the other hand 40 of our genera common to Europe are represented in our flora by 9 or more species (not excluding the naturalized ones), and the 34 larger genera average as much as ten indigenous species apiece.

As to the relative number of species in our 34 largest amphigæan genera, it may be interesting to note that their sum in our flora is 637 species; in the Flora Germanica of Koch, 621;—the naturalized plants not being excluded; but these are quite as numerous in the German flora as in ours. Also 20 of these genera are larger in our flora than in the German. If the admitted species were brought to a common standard, the numbers would tell more decidedly in our favor. The large genera of which we possess the superior number of species are *Carex* (132 in our flora to 109 in the German), *Aster* (38 to 8), *Solidago* (35 to 1), *Panicum* (20 to 7), *Polygonum*, *Cyperus* (19 to 7), *Quercus* (18 to 5), *Eupatorium* (16 to 1), *Platanthera* (16 to 2), *Eleocharis* (16 to 7), *Hypericum*, *Polygala*, *Vaccinium* (11 to 5), *Utricularia* (11 to 4), *Scutellaria* (10 to 4), *Rhynchospora* (10 to 2), *Glyceria*, *Rubus*, *Viburnum* (10 to 3), and *Smilax* (10 to 1).

(*To be continued.*)

ART. XI.—*Observations on the Falls of Niagara, with reference to the changes which have taken place, and are now in progress;* by R. BAKEWELL.

IN a recent work entitled "A Manual of Coal, and its Topography, by J. P. Lesley, Topographical Geologist," the author under the head of "Theory of Denudation," makes some remarks which are intended to shew that the Falls of Niagara have undergone little or no change during the last one hundred and seventy-eight years,—with the exception of the lateral fall by Table rock, represented in Father Hennepin's drawing taken in 1678,—and he draws the conclusion that the ravine from Lewiston to the Falls was not caused by the action of the river. He says that "the only change in the Falls since Father Hennepin's day is the diversion of the small oblique cascade from Table rock, and the occasional droppings of the ledge, amounting perhaps in all to a few hundred tons a year." He refers in further corroboration to M. Desor, whose "experience in the Alps led him to feel how refractory such a plate of limestone rock must be, over which all the waters of the lakes might storm for generations without producing a change determinable by ordinary triangulation." Mr. Lesley, in referring to some observations which I had published in the years 1830 and 1846, says, that I was justified by reference to Father Hennepin's drawing in 1678, and Kalm's in 1754, and from reports of old men living in the neighborhood, in assigning three feet per annum as the rate of retrogression of the Falls. In the years 1829, '46, '51 and '56 I visited the Falls. During my visits I have taken many sketches at different intervals of time, and have noticed with much interest the changes which have taken place since 1829, proving conclusively I think, that there is a slow *onward* retrocession of the Falls—that the cataract of Niagara once poured over the precipice at Lewiston, as I have endeavored to shew by description and diagrams previously published. My object in this communication is to corroborate the views there entertained, and to exhibit, *not* what the Falls are supposed to have done in ages past, but what they are *now* doing. And let me first say, that my allusion to Father Hennepin's drawing had reference solely to the *noise* then produced by the Fall, as shewn by the hands of one of the persons in the view being placed on the ears; that further, I never saw the drawing by Kalm; and as to the reports of old men,—when at the Falls in the year 1829, I was informed by Mr. Forsyth, the proprietor of the Pavilion Hotel on the Canada side, that during his residence of forty years the Falls had receded forty yards.

Moreover, with regard to the refractory limestone rock, it is well known and has often been stated that the limestone at the Falls rests on soft shale, only half of the height of the Fall being limestone, and this limestone is carried off, not by the action of the waters on it, but by being undermined, through the erosion of the shale, a rock full of joints, and therefore separating into blocks. The limestone falls away readily when through the undermining action it is left without support. We see therefore no force in the citation from M. Desor.

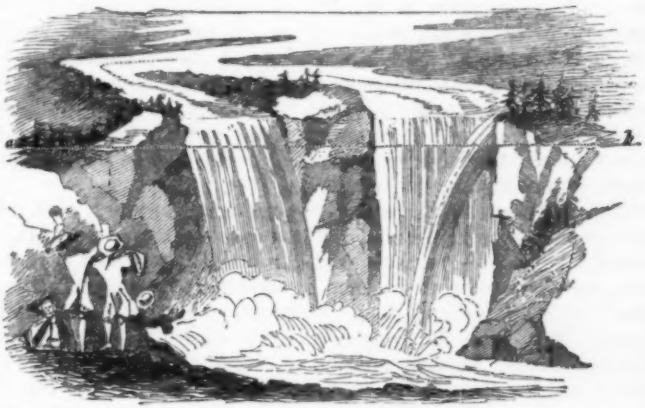
Every little torrent has its furrowed channel, and often its deep pot-holes, as a result of the action of the water, and it would be most strange, if the great flood of Niagara, should rush on its course for ages and produce no appreciable effect.

A few words on these pot-holes, and the great pot-hole of Niagara, before I proceed. Some of these excavations or pot holes I have measured in situations where there is little water flowing except in the spring of the year. There are three at the Bashapish Falls near Mount Washington, in the state of Massachusetts, succeeding each other in a narrow ravine within the space of forty yards. One of them was twenty-two feet in depth, independently of the debris at the bottom, the depth of which I could not ascertain. The diameter of this circular basin was about three yards. If then, a stream of water, active only a short period of the year, is capable of making such inroads in the hardest rocks, are the Falls of Niagara the only exception? Where, it may be asked, are the pot-holes of Niagara? I would reply: walk on the precipice of the chasm from Lewiston to the Falls, for an answer. Then, in imagination, let the flood above the rapids be held back, the river dried up, the whirlpool emptied of its contents; and then we shall have a magnificent pot-hole, the largest in the world, with two deeply fluted arms stretching north and south, each three miles in length. As I was standing a few yards south of the summer-house at the whirlpool this summer, I was impressed more than ever with what was taking place below me, especially in the very narrow channel through which the rushing torrent darts into the whirlpool. I regard this as one of the most interesting features connected with the Falls, and yet the least thought of by the thousands who visit the place. The gorge through which the waters rush is very narrow: I was told in 1846 by the proprietor of the place that the width was only 72 yards. This I believe to be nearly correct, for at the suspension bridge, which is at the commencement of the rapids, and where the river is much wider, I found by measurement on the bridge that the width was only 118 yards. I noticed that underneath the water, a portion of rock from each side extends, as well as I could judge, about five yards from each side of the entrance into the whirlpool, reducing to sixty-

two yards the distance through which is flowing every minute the drainage of four great lakes, covering an area of about 135,000 square miles. The waters escape at the northern extremity of Lake Erie through a channel (as stated by Mr. Allen from measurements by Mr. E. R. Blackwell) 1,700 feet in width, 32 feet in depth, running at the rate of six miles an hour, equal to 22,440,000 cubic feet, weighing 701,250 tons; here the whole is confined to a breadth not exceeding 225 feet, and then it suddenly becomes sluggish at the whirlpool, (fig. 2, *h*, entrance into the whirlpool). The solution is found in the structure of the rocks. The very hard bed of quartzoze sandstone on which the debris at the rapids rests, is here broken through, under which lies a very soft red shale,* which has been scooped out by the resistless torrent from each side underneath this rock to an unknown width and depth. A passage is thus made for the flood to escape. The red shale crops out at Lewiston.

The annexed outline of Father Hennepin's drawing is carefully reduced from the lithographic print found in Lyell's Travels in North America in the years 1841 and 1842. From the original Utrecht edition, 1697.

1.



The drawing must have been taken from a high position, and on the Canada side of the river. Without regarding the straight line on the verge of both falls, it seems to teach that a change more great and surprising than poets yet have feigned has taken place. By measurement, the present distance across the American Fall and Goat Island to the commencement of the Canada Fall is, according to the guide-books, 430 yards, to which, if we add only one-third of the Canada Fall we shall have in all 630 yards.

* This is not the shale under the limestone as seen at the Falls.

The height of the American Fall is 167 feet. We thence have the breadth equal to eleven times the height of the American Fall. But in the drawing the same distance equals only two Falls, although it represents a front view. To form a more correct idea of the relative proportion of the breadth and height of the Falls and Goat Island as they now exist, I have introduced the line (*a*, *b*) across the drawing. Thus faulty is the view on which arguments have been based respecting the rate of recession.

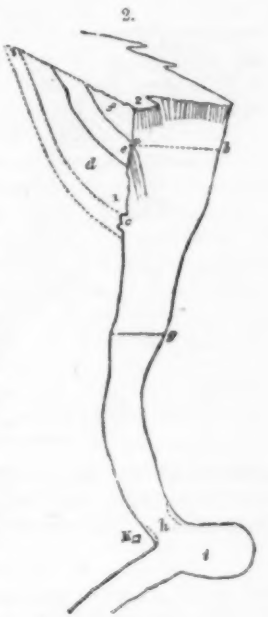
Father Hennepin modestly complains of his want of skill in description. He writes "J'ay souhaité bien des fois en ce temps là d'avoir des gens habiles à decrire ce grand et horrible saut." Describing the waters on each side of the Island (Goat Island) approaching the precipice—"Et c'est alors qu'elles se précipitent dans une abyme, qui est au dessous à *plus de sixcens pieds de profondeur.*" The height of the Falls he gives as more than 600 feet—more than three times their present height. The lateral fall as seen in profile must have been more than one-fourth of the width of the American Fall.

Mr. Lesley refers to M. Desor, who writes: "that such a rapidity of action by water on rock as three feet per annum (referring to my former statement) is unnatural." He argued that "if we grant the indentation in the American Falls which is only 132 feet back from a straight line across, to have been made since Father Hennepin's time 178 years ago, it only gives a partial recession of nine inches per year and an average of only half that or four and a half inches per year." Taking the recorded falls of Table rock as a basis of calculation, the British Falls could not have touched the American Fall at right angles less than six hundred and fifty centuries ago.

The American Fall has not the material to work with, that belongs to the Canada Falls. In one case the water is but thinly spread over a wide surface. In the other, the water is carried over one-third of the distance in one vast unbroken mass of great depth and volume,—an exhibition of power immeasurably beyond that of the American Falls. We have no definite measurement of the relative amount of water pouring over the two Falls; but we should not be far from the truth in saying that the water descending the Canada Fall is fifty times greater than that of the American. But, again, the inquiry may be made, what were the American Falls doing all these 65,000 years? Whence the necessity of supposing that the American Falls had any existence when the Canada Falls were where the American Falls now are? Six hundred of these centuries may have passed in which the Canada Falls were toiling night and day *alone* in that vast wilderness. It is highly probable that a slight deviation in the channel of the river above the rapids, carried off the waters on the American side more to the northeast, thus producing the American Fall, as may be seen by referring to fig. 2.

The dotted lines from *a* to *b* may represent the position of the Falls when at right angles with the American Falls (*e*) agreeably to the supposition of M. Desor. But, as I have before said, unless it can be shown that the American Falls *did exist* when the Canada Falls were at *a b*, and continued afterwards to exist, all the calculation based upon the slight indentation backwards in the outline of the American Falls amounts to nothing.

The following facts show that the probabilities are, that the American Fall is comparatively a recent diversion from the main channel, that when the Canada Fall was in the vicinity of *a*, there was a lateral fall at the cave (*c*). The dotted lines representing the course of the supposed river, Goat Island would then include the space in the triangle 1, 2, 3, the American Fall thus having no existence. If the dip of the bed of the river were to incline more to the west, or become deeper it would soon draw off the waters of the American Fall, and we should then have in its place a lateral dry bed, or channel, a repetition of what I believe formerly took place when the flood poured over the precipice at *c*, at a distance of about half a mile from the American Fall. Here, for more than 100 yards, the rock is quite bare, has a water-worn appearance, and presents a very remarkable outline on the edge of the precipice, reminding me the moment I saw it of the ragged outline of the rocks at the brow of the American Fall. It is interesting to compare one with the other. The resemblance is so similar in form as well as depth of indentation, that we might reasonably infer that the time spent on both in wearing and breaking down the edge of the precipice was nearly the same.



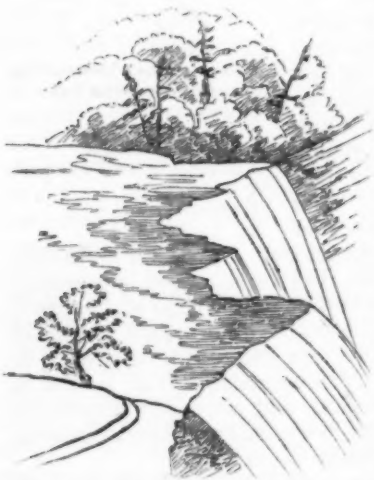
c, the cave.
d, elevated bank between the cave and the American Fall.
e, American Fall.
f, Goat Island.
g, Suspension Bridge.
h, entrance into the whirlpool.
i, whirlpool.
k, summer house.

Fig. 3 is an outline sketch of the cave about half a mile from the American Fall, taken in 1846. (See fig. 2, *c*.) At the farther extremity of the cave was a small stream of water flowing down the precipice—the remains perhaps of the magnificent flood which once awoke the echoes of the place. In 1856 this had disappeared. The pure cold spring which gushed from the rock

3.

near by is still there. The outline of the American Fall is represented by fig. 4. It is reduced from an extended sketch I took from the top of a Pagoda, standing near what is called "Point view" (in 1846) which explains the map-like appearance it assumes. On passing the bare ledge of rock at the cave we come to a bank of drift thickly wooded, (fig. 2, *d*,) which continues on the edge of the chasm until we approach within a few yards of the American Fall, *e*, these few yards exhibit a zig-zag appearance on the precipice, and bare rock, as if the Fall had once been there, and indicates a movement sideways as well as backwards. The resemblance of this wooded elevation between the cave, *c*, and American Fall, *e*, suggests the high probability that it was once surrounded by water, and assumed the appearance which Goat Island now presents, the flood then going over the precipice of the cave, (*c*). The dotted lines represent the former river—the bed of which I did not trace back to its commencement, although I felt convinced in my own mind, that it did exist. As I had no thoughts of writing on the subject at the time, I unfortunately let the opportunity escape. But it gives me pleasure to say that since the above was written I have unexpectedly conversed with Mr. P., the very intelligent proprietor of this princely domain, and it is his politeness to an entire stranger which enables me to speak with more confidence ou this interesting point. I mentioned to him what my opinion was respecting a torrent having once poured over the precipice at the cave,

4.



and asked him if there was any indication of a bed of a river in the neighborhood of the Falls. Mr. P. informed me that *there is the appearance of a former bed of a river back of the village.* This information enables me with more confidence to trace out by the dotted lines, where it ran. He also informed me that a slight inclination of the bed of the river west of Goat Island had retarded the encroachments made by the current, and that he had thrown out breakwaters for further protection. He observed that the principal changes going on were on the Canada side, and he agreed with me in thinking that the waters were concentrating more into the centre of the Horse-shoe Fall. The falls instead of facing the north, as at present, will, as the waters gradually retire from the Canada side, make a right angle with the river and face towards the northwest, and contract their dimensions within another *narrow* gorge. It is a curious fact which I noticed along the course of the ravine that there are many of these contractions and expansions. There are two from the Falls to the Suspension Bridge, and there are several from the whirlpool to Lewiston.

When the Canada Fall has turned the angle which I think it is now making, the breadth of the river will then be greatly reduced in width, increasing its velocity and depth, causing in all probability the waters of the American Fall to be drawn off, leaving the Canada Falls once more to toil alone.

I was further informed by Mr. P. that the American Fall has been protected from erosion by the rocks which are exposed and piled up at its base. I would in connection with this, again advert to M. Desor's calculation as to the retrogression of the American Fall, to show that if the same method be adopted with the Canada Fall, we shall have a result very near to the statement of Mr. Forsyth before alluded to, viz., that the Falls had receded forty yards in 40 years. If a line be drawn across the Canada Fall from Table rock to Goat Island touching the water on each side, and a perpendicular line to the extremity of the indentation, as found in Prof. Hall's survey of the Falls of Niagara, we shall have a distance equal to 912 feet. Taking the width of the river at the ferry which is said to be 1,254 feet, as a scale of measurement, we shall then have the following result. To the extreme point of indentation 912 feet: half of this for an average depth of the whole Fall, will give 456 feet of recession in 178 years, rather more than $2\frac{1}{2}$ feet per annum. This comes from following the data in Father Hennepin's drawing. But it cannot be supposed that there was no indentation at the time he made the drawing. Prof. Hall has justly remarked that, "in the absence of established landmarks, we are compelled to leave the rate of recession unsettled for the present. The accompanying trigonometrical map of the Falls will furnish the means of doing this, by the monuments which have been estab-

lished, and which may be considered as permanent points of reference for the future."

Leaving, for the present, all speculation respecting the origin of the chasm extending for seven miles, let us look at the Falls as we *now* find them, and the ravine through which the waters rush on their way to Lake Ontario. On walking from Table rock by the precipice to the whirlpool, the high bank of drift to the left becomes less elevated, and retires a little farther to the west. At the whirlpool it caps the limestone at the precipice, through which there is the bed of a stream and over which during the spring of the year, and during heavy rains, the waters rush over the precipice. Passing this ravine we again ascend over the drift before we descend to the level of the whirlpool, when a magnificent scene opens upon us. Here, we have at one view the high wall-like appearance of the rocks stretching above and below the whirlpool on the American side. On the Canada side two bold promontories mark the entrance and the outlet of the whirlpool. These are thickly wooded down to the water's edge and encircle the scene.* On passing the ravine which is partly filled with drift, extending, as we are informed by Mr. Hall, to St. David's, we continue for a short distance along the margin of the whirlpool before we ascend the projecting rock which hangs over the outlet. The surface of this rock is denuded of drift for a short distance from the precipice, and the surface is very level. I noticed that with one exception, the precipice rises immediately from the river. Where this occurs, an amphitheatre is formed larger, I think, than at the whirlpool, the floor of which is wooded leaving the breadth of the river nearly the same. On passing round this semi-circle, the high drift again comes to the edge of the precipice, and through the drift a dry bed of a stream tends towards the precipice. The rocks on each side, as far as the rapids below the whirlpool extend, preserve about the same distance from each other as they do at the rapids above the whirlpool. The torrent for more than a mile rushes with the same impetuosity over hidden rocks as before, when it gradually subsides and moves on smoothly, but swiftly towards the outlet. On approaching the end of this gorge the drift again is found capping the limestone on which Brock's monument is erected.

* A very interesting incident took place as I was engaged in making a sketch of the scene. A beautiful little bird flew over my head, and perched upon a log of wood which was floating along on the outskirts of the vortex of the pool. I noticed that it did not appear satisfied with its position, and watching it very attentively, I observed (for it was not far from where I was stationed) that having sailed on the log a short distance the bird flew to a piece of wood which the motion of the water caused to tilt up and down. Having secured its hold on one end, it disappeared under the water. As the end of the stick rose with the bird to the surface, the bird fluttered its wings. This was thrice repeated,—after which it flew away.

On the American side, I have followed the ravine from the whirlpool to the Falls, the most remarkable feature is the abrupt turn which the river makes, almost at right angles with the ravine below the Falls. Here is a high bed of drift resting on the limestone.

I will now consider some of the changes which have taken place during the last twenty-seven years. In doing this I do not rely solely on my memory. I have my sketches to bear me out. In 1829 the whole of the Canada Fall was entirely covered with water, no bare rocks were seen as now, peeping out to indicate what was taking place. From below where the tower now stands, the most beautiful feature of this Fall was caused by the obstruction which the flood met with from projecting rocks, causing the water to *boil* over in magnificent white globes, increasing as they descended, at the same time entirely covering the obstruction. A similar obstruction would not now produce the same effect for want of water. The Fall, which was then more circular in form, now assumes a wedge-like appearance, and the deep indentation points to the American side of the river. The center has receded many yards from its former position—not that the whole has gone back, but only where the waters to the depth of 30 feet wheel over the precipice. On the Canada side of the Fall the water is not so deep, is more broken, and falls over more languidly than formerly, as it approaches what was once called Table rock. I miss another exceedingly beautiful appearance, similar to the one before alluded to on the American side. There was then a lateral stream which fell over in the direction denoted in Father Hennepin's drawing (fig. 1). This stream, in falling over the rock, was tossed about by the current of air sweeping from underneath the Fall. In 1846 it was not there. There is, indeed, now to be seen a lateral stream issuing from a hole in the rock; but this is the refuse water of the engine house which is pumped into a reservoir on the high bank. In 1829, as I was standing on Table rock, attempting to take a minute sketch of the Canada Fall, my india rubber dropped from my hand; I made a snatch at it, but it fell down the precipice. The consciousness of my danger, had I made a step in advance, compelled me for awhile to lay aside my drawing. This rock, the principal "stand point" at Niagara is now laying at the base of the Falls, an additional monument to "record the mischiefs they have done."

When at the Pavilion House in 1829, which was situated on the high ground west of Table rock, I made a panoramic sketch from the top of the house. I then saw and represented on my sketch a small bare gravel-patch situated about one-fourth of a mile west of the centre of the Falls, and distant from the Canada side one-third of the way. In 1846 this patch had considerably increased, and some bushes were growing on it. In 1851, when

I made another sketch of the same scene, it had become much larger, its longest diameter being about north and south. In 1856 the bushes had become young trees. Having nothing to compare it with, it is difficult to get a correct idea of the size of this new formed island. Judging from my sketch, I should say that it was about 100 yards long and 20 wide. If correct, the island of 1856 is five times the size of the gravel patch of 1829. My object in being so particular, is to show that there is a gradual and perceptible change going on, drawing the waters from the Canada to the American side, or rather, to the centre of the Fall. To this it may be replied that the increase was simply by deposition. The difficulty in this case would be to arrest the particles of sand and gravel in the midst of such a current. In review of some of the changes now going on, we have immense portions of rock now in sight which *once faced* the precipice at the Falls. We have on record at the present day that there has been a great disruption from falling rocks, hid from sight in the deep bed of the river, which have changed greatly the former outline of the Fall,—the retiring of the waters from the Canada side towards the middle of the river greatly increasing the power at work in wearing away and breaking down the precipice over which they plunge. The face of the Falls assumes a very different appearance from what it bore in 1829, scarred as they now are with deep lines of rock which may serve to indicate change though not enhance beauty.

If then, in the short period of twenty-seven years, so much has been done to change the character of the Falls, is it reasonable to suppose that they present now the same appearance as they did in Father Hennepin's day?

I would make an inquiry of those skilled in acoustics, why it is that the immense body of water which pours over the centre of the Canada Falls is comparatively so noiseless. On thinking of this when last at the Falls, I was more deeply impressed than ever with the fact. We hear the rush and turbulent noise of the waters of the rapids and of the American Falls, and the two sides of the Canada Fall. But that low, heavy, muffled sound is not the deafening roar which we should expect. The only explanation which occurs to me is, that the flood pouring over the precipice *unbroken*, continues unbroken until it reaches the bed of the river, there, first meeting with resistance *under* water, the sound is conveyed away *beneath*, or with *the flood*.

A person with his head under the river at the ferry would, I think, experience a fearful stunning sound. I regret that I did not try the experiment. If the water could for a moment be suspended, the first plunge after, would doubtless cause a terrific noise from the act of breaking the surface of the water; but when this was once broken, and the descending flood was in

continuous flow, would not the noise be far less although still powerful beneath the water? I think that the drum must have been unmuffled when Father Hennepin visited the Falls.

In closing these remarks I would express the deep interest which I always take in this magnificent scene. Years have passed away since my first visit, and with them the recollection of much that I have seen of the grand and beautiful in nature. But my impressions of Niagara have not been effaced. And the more I am absorbed by the glorious view, the more does my feeling of its grandeur lose itself in admiration of its beauty.

New Haven, 1856.

ART. XII.—*Biography of Johann Nepomuk von Fuchs;* by FRANZ VON KOBELL.*

* * * JOHANN NEPOMUK VON FUCHS was born at Mattenzell near Brennburg in Bavaria on the 15th of May, 1774. He studied first for the clerical profession, but afterwards gave his attention to medicine, and graduated in this science at Heidelberg in 1801. A residence in Vienna led him to begin the study of chemistry under V. Jaquin, and his liking for this science and for mineralogy became so great that he abandoned medicine, and, supported by his government, went to Freiberg where he heard the lectures of Lampadius and Werner, and made himself familiar with mining and metallurgy. His intercourse with his friend, the celebrated crystallographer, S. Weiss, who was his fellow student at Freiberg, led him to cultivate mineralogy with special zeal. From Freiberg, he went to Berlin and continued his studies under Karsten, Klaproth and Valentine Rose, the latter of whom took a deep interest in his progress. He then made a short stay in Paris where he acquired the esteem of Hauy, as is shown in the correspondence of the great French mineralogist, even where in one case he controverts the views of Fuchs.

In 1805, the subject of our memoir settled at Landshut as *Privat-Docent* in the University there, and in 1807 he was appointed Professor in ordinary of chemistry and mineralogy. At that time he suffered so much from weakness of the chest that no one thought he could survive many years, and although this difficulty never quite left him, he had such confidence in the strength of his constitution that he manifested little anxiety about working in an atmosphere filled with acid vapors and noxious gases.

* Read at the Public Session of the Academy of Sciences at Munich, on the 28th March, 1856. Translated for this Journal by Prof. S. W. JOHNSON, of the Yale Scientific School.

In 1823, he was called to Munich and to the Academy of Sciences as Conservator of the national mineral collections, and when in 1826, the University was transferred from Landshut to Munich, he again entered the corps of professors, and instructed in mineralogy.

In 1833 he was appointed to the Higher Medical Commission, and Counsel of Instruction, and in 1835 he was named Chief Mining and Saline Counsellor, at the same time retaining his previous station. His new duties however were not of a kind compatible with his devotion to investigation, so that he shortly desired to return to his old pursuits, and in 1844, in full acknowledgment of his extraordinary ability he was pensioned as Chief Mining Counsellor, and was thus enabled to pursue undisturbed his manifold studies, which occupied him until 1852, when, at the age of 78 he was pronounced Privy Counsellor, and laid aside his active employments. Fuchs left behind him at his death a widow and one son.

The first considerable research of Fuchs was concerning the Zeolites, which he analyzed partly in connexion with Gehlen. This investigation was broken off by the death of the latter who perished from accidental poisoning by arseniuretted hydrogen, in the year 1815, and Fuchs was so dejected at the loss of his friend that he would never have resumed it but for the severe criticisms of Hauy upon some of the results which had been published.

Fuchs showed that the crystallization of the *Mésotype pyramidée* was not quadratic as Hauy had assumed, but was rhombic, and that Hauy's *Mésotype épointée* was no mesotype at all, but apophyllite. For the first, the difference of angle was only 1°, but this was an important difference, while, in case of the *mésotype épointée*, chemical analysis sufficiently demonstrated its identity with apophyllite. Fuchs also had announced Scolecite as a new species, which Hauy was disposed to doubt, partly from the similarity of its crystallization with his *mésotype pyramidée*, and partly because he considered the old analysis of Vauquelin, who like Fuchs had found lime instead of soda, to be incorrect from imperfections in the analytical method. The results of Fuchs were confirmed, and this controversy contributed to his reputation. In another investigation relating to Aragonite and Strontianite, he also discovered some inaccuracies in the determinations of Hauy, and first directed notice to the great similarity of crystallization between aragonite and strontianite, witherite and carbonate of lead, as also between heavy spar, celestine and sulphate of lead. Fuchs was then inclined to explain this similarity of form by the fact that these groups contain a common member; viz., their acid ingredient. If we combine these observations with an earlier paper of Fuchs, which he wrote on instituting the new species gehlenite, we find the elements of the doctrine of isomorphism that was subsequently announced by Mitscherlich.

In his paper on gehlenite, Fuchs considers the peroxyd of iron which that mineral contains, "not as an essential constituent of this species, but as only a *vicarious ingredient*, if such an expression be permissible,—a substitute for nearly the same quantity of lime, which in the absence of the peroxyd of iron should be present to maintain the stœchiometrical relations of the mineral," and he remarks, "I believe that varieties will be found which contain much less or no iron, and a correspondingly greater amount of lime." In the same article he mentions that sulphate of alumina can form alums, not only with potash, but with ammonia also, or with both together, and that Gehlen had also prepared a soda alum, which fact Fuchs connects with the composition of albite wherein soda replaces potash. How fully he recognized the value of this observation is evident from the following remark: "From this point of view we see that it is requisite to compare the results of many analyses of mineral bodies, if on the one hand we will harmonize their constitution with the doctrines of chemical proportions, and on the other hand prevent an unnecessary multiplication of species." In the same paper he instances correctly the ratio of the ingredients of vesuvian, although he likewise assumes that peroxyd of iron replaces lime. The reason that Fuchs did not then remark that only bodies of analogous constitution can replace each other, doubtless lay in the small quantity of the peroxyd of iron present, so that the difference between it and protoxyd was too slight to direct attention to the fact that the sesquioxyds cannot replace the protoxyds. The merit of Mitscherlich in developing the doctrine of isomorphism is unmistakable, but it were unjust to the memory of Fuchs to deny that by the above observations he first excited attention to this subject, and laid the foundations of our knowledge concerning it.

In 1816, Fuchs published a preliminary notice of some mineral phosphates, and two years later followed his researches on Lasionite and Wavellite.

These investigations distinguished him as a profound analyst. He found that a mineral from Amberg which he named Lasionite, was a compound of alumina, phosphoric acid and water, and after he had overcome the difficulties in separating phosphoric acid and alumina, by an entirely original method, viz., the application of silicate of potash,—he undertook the analysis of wavellite, suspecting that in it, phosphoric acid had been overlooked. In his paper he says—"The thought indeed occurred to me that wavellite which I had never yet seen (at the time of examining Lasionite) could not be very different, but I dared not entertain this idea against the testimony of three most celebrated chemists, Klaproth, Davy and Gregor, who had analyzed it and found noth-

ing in it but alumina and water. Since, however, I have become satisfied," &c. His analysis in fact proved the identity of these minerals.

Already Fuchs had remarked that silica forms compounds in the wet way which perfectly correspond with some that occur in nature. The further study of these bodies continued to occupy him, and therewith are connected his later researches on the formation of porcelain clay, on soluble glass, and hydraulic lime. In his paper on Lazulite, besides proving in this mineral the presence of phosphoric acid, which had been overlooked by Trommsdorf and Klaproth, Fuchs makes the first mention of the fact that various insoluble silicates, viz., lazulite, prehnite, zoisite and vesuvian, are decomposable by acids after strong ignition.

In 1821, Fuchs analyzed wagnerite which had been previously confounded with topaz, and showed it to be a compound of phosphate of magnesia and fluorid of magnesium.

In the train of these researches appeared that on the formation of porcelain clay. At that time it was thought that potash feldspar was the material from which this clay was produced. Fuchs first showed that clay may originate from other minerals, and proved that the porcelain clay of Passau results from the decomposition of the so-called porcelain spar, which, it is remarkable, has hitherto been found in no other locality. So much vagueness still exists among mineralogists with regard to the process of weathering, that it may not be useless to recall the explanation of Fuchs. He says: the process of weathering is something similar to the spontaneous decomposition of organic bodies, and has been not unaptly compared with fermentation. Hence porcelain clay has a constant composition, and for this reason we must consider it as a distinct species, and not merely append it to porcelain spar, as Hauy, in the belief it originated from feldspar, has attached it to that mineral, under the name *Feldspath-Decomposé*. The porcelain clay has no more in common with porcelain spar, than alcohol has with the sugar from which it is derived, and it sounds just as strangely to call the clay decomposed porcelain spar as it would to designate alcohol as decomposed sugar. On occasion of these researches, Fuchs analyzed the precipitate first observed by Guyton Morveau, which falls when potash solutions of silica and of alumina are mingled together; and found that it contains a considerable quantity of potash, and that when it is treated with lime it gives up potash, and forms a compound similar to scolecite. In this connection, he observes, with regard to fluxing silicates by fusion with alkalies or other bases, that this process consists in the formation of "a new body, a new mineral, so to speak, which is similar to or identical with those natural compounds that are dissolved or decomposed by acids." Already in this paper occurs a passage reminding of the

views of Fuchs on geological changes and his reaction against Plutonism. It reads—"Since, according to the foregoing observations, silica and alumina together, are a means of precipitating the fixed alkalies from their solutions, we have a way of explaining how potash and soda could be removed from the waters of the ancient world, and enter into the composition of feldspar, mica, &c., a consideration that would be very important to the geologist."

Fuchs always devoted great attention to blowpipe experiments, and to him we owe the characteristic reaction of phosphates; viz.: the greenish tinge they communicate to the flame after moistening with sulphuric acid; as well as the distinction between potash and soda by means of the yellow or reddish-violet colors they respectively impart to the flame of the blowpipe. The reddish color which spodumene gives the flame had fallen under his observation before the discovery of lithia, and I remember his expression of chagrin that he had omitted to investigate the cause of this coloration, for he was convinced that otherwise the discovery of lithia would not have escaped him. On the 27th of March, 1824, Fuchs delivered the customary oration before the Munich Academy of Sciences, and chose for his subject—the reciprocal influence of Chemistry and Geology. He showed that a great gap would be formed in chemistry should mineralogy be separated from it, and that the conclusions of the chemist cannot be drawn from chemical relations alone, but that he is greatly and indeed chiefly dependent upon the external or physical characters from which we first derive our conception and knowledge of bodies. He illustrated the importance of crystallography, and reminded how the chemist is often obliged to combine a problematical body with other substances, in order to recognize it by means of the physical characters of the product. Thus, alumina is united to sulphuric acid and potash, and then decisively made out from the octahedral form of the resulting alum. As thus, the physical properties of bodies serve the chemist, so in a much greater degree, the chemical characters assist the mineralogist. Fuchs asked—what would mineralogy be without chemistry? and replied—without chemistry, mineralogy would be but a chaos. She could never have assumed a form at all satisfactory or consistent with just expectations. Only a glance at the history of mineralogy is needful to see that it has not become what it now is by innate strength; it never could have learned the whole breadth of the applications of crystallography, even but by the help of chemistry. I am convinced, says he, that without the latter science we should to-day be seeking to find specific distinctions for minerals in the confusion of accidental forms and aggregations, in unimportant colors, degrees of transparency, &c. Minerals of crystalline, massive and

earthy formation would be ranked together, and the mineralogist would go idling about among accumulations of moulds and marls among numberles varieties of clay, sand, wacke, &c. Who would have ventured to unite rock-crystal with flint, or calc spar with chalk, had not chemistry proved their essential identity? Fuchs denounced the so-called natural history method of Mohs, saying that it is mere fancy, not law:—it is not written in the book of nature that mineralogy has to deal only with the obvious external characters of minerals. The object of the science is to learn to recognize and distinguish minerals, and to acquire a comprehensive and thorough knowledge of them. This object cannot be attained without the assistance of chemistry.

In 1825 appeared a paper on soluble glass, a substance which has become useful in several ways, and still presents various interesting points for investigation, especially the nature of the precipitates it produces in solutions of metallic oxyds. Fuchs had already at that time directed notice to these precipitates, and had mentioned as characteristic the blue color which is obtained with cobalt salts. The first industrial application of the soluble silicate of potash was for rendering edifices fire-proof, and was made in 1824 on the then newly erected Theatre in Munich, after the necessary preliminary trials had shown the process to be satisfactory. The chief advantages of this substance lie in this, that it exerts no detrimental action on the combustible to which it may be applied, but rather serves eminently to shield it from damage by other causes, forming in fact, when properly prepared, a perfectly coherent and very durable coating, which suffers no change from meteoric influences, and furthermore is inexpensive. Fuchs showed that it might be used to make linen fire-proof as well as wood, that it could be employed in preparing artificial stone, and for cementing glass and porcelain. He also proved that the presence of lime and metallic oxyds in common glass is not so unimportant as had been supposed, and that from a pure silicate of potash or soda a durable glass cannot be manufactured. To what a degree the applications of soluble glass have recently been increased, is learned from a late account by Baron Liebig, wherein he mentions the extensive fabrication of it carried on by his friend Kuhlmann at Lille, and describes how it is used in enormous quantities to prevent the decay of walls, dwellings and churches, even when built of very inferior stone, besides being employed in print-works and tapestry-factories to fix colors upon cotton and paper. Liebig expresses himself "astonished and ashamed at this—ashamed because in Germany soluble glass exists only in chemical treatises and because he knew with what disappointments Fuchs was obliged during many years to contend, before he could see realized but one of its many useful applications." In immediate connection with

the discovery of soluble glass, stands the art of *Stereochromy*, a a style of fresco-painting invented by Fuchs, a complete account of which he wrote but a short time before his death and left behind him, as an invaluable legacy to art. Kaulbach has the merit of first recognizing the advantages of this method, and putting it in use. His great frescoes on the New Museum at Berlin are stereochromically painted, and very recently we have seen specimens of this kind executed by Echter, for the Strasburg Minster. They are perfectly successful, and, it deserves to be noticed, are painted directly upon the sandstone which is used as building material. In carrying into practice the idea of fixing colors by means of soluble glass, numberless difficulties presented themselves. It was found, for example, that several new pigments must be invented. Fuchs finally succeeded in this, and among others he produced a white pigment of the finest quality, and especially faultless in its most important application, viz.: for mixing with other colors. This color is now used in oil painting. Stereochromy has decided advantages over ordinary fresco and encaustic painting, for the artist enjoys full freedom in laying on his colors, he is not confined to any limited time, he can alter and improve at will, because the fixing of the color is done after the painting is finished, and is to a certain degree a technical operation, which may be left to ordinary workmen. Numerous experiments have demonstrated the unchangeableness and durability of these pictures, many specimens of which have been purposely exposed to the severest trials without suffering injury. In 1851 Fuchs received a prize medal at the Great Exhibition, for this invention, and before his death, had the satisfaction to see stereochromy appreciated in his native land, and especially by his majesty, the reigning King Maximilian.

(To be continued.)

ART. XIII.—*On the Presence of Fluorine in the Blood;* by JEROME NICKLÈS.

FOR reasons which I shall soon have occasion to present, I have been led to verify the assertion so much contested, of the existence of fluorine in bones. My results having been in the affirmative, I next looked for fluorine in the blood—the only means by which it could penetrate into the osseous tissues. I have found there notable proportions, not only in human blood, but also in that of several Mammalia, (as the sheep, ox, dog,) and several birds (the turkey, duck, goose, hen).

Results so concordant, seem to give to fluorine an importance which it has not yet had in medicine and physiology. They

set aside the opinion of Berzelius that the presence of fluorine in the bones is purely accidental and not in any case a necessary ingredient. If we wish other proof of the necessity of reconsidering the conclusion of this illustrious chemist, we have them in the following facts: that fluorine exists in the bile, in the albumen of the egg, in gelatine, in urine, in saliva, in hair; in a word, the animal organization is penetrated by fluorine and it may be expected to be found in all the liquids which impregnate it.

In view of these facts, which I have verified with exactness and all possible care, it is evident that fluorine plays in the blood and other liquids of the system a physiological part. Its absence or its diminution must constitute of itself a state of disease, a species of chlorose from the absence of fluorine, analogous to the chlorose from the absence of iron. This disease may be detected no doubt by a chemical examination of the urine or saliva, and may be met by a fluorid preparation. Thus far, my own experiments have been made only on normal urine, from an adult in perfect health or from healthy children.

On another time I will make known the simple process by means of which I have been able to recognize the presence of fluorine in all kinds of substances.

Nancy, Oct. 30, 1856.

Art. XIV.—*Correspondence of M. Jerome Nicklès, dated Paris, August*, 1856.

Obituary of Gerhardt.—The same week witnessed in France and England the death of two distinguished geologists, Buckland in one and Prévost in the other, both more than seventy years of age. These two learned men lived long enough to see the triumph of their views, and reap the fruits of their labors. But what have we to say of the young man, strong and full of health, who but yesterday made a great sensation by the profundity of his conceptions, the novelty of his ideas, and the brilliancy of his discoveries, and who has fallen at the age of forty years, almost without having been ill? Gerhardt closed his eyes the very day after giving up the last proof-sheet of his *Traité de Chimie Organique*, a continuation of the work of Berzelius. On the 19th of August, in consequence of an acute peritonitis, he died at Strasbourg, where he occupied the Chair of Chemistry in the Faculty of Science.

Born at Strasbourg, Aug. 21, 1816, he was destined by his father for trade, as a manufacturer of white lead; but like Laurent* he showed no liking for that career, and made every effort to extricate himself. He had acquired some notions of chemistry at the gymnasium of Strasbourg, where he had studied, and often applied himself to questions

* See the biography of Laurent in this Journal, Aug., 1853.

which the state of the science did not enable him to settle. At eighteen years of age he was directed by his father to travel at the expense of a business house for the sake of selling their manufactures. This employment of traveling clerk, though not congenial to him, gave him an opportunity to satisfy partially his inquiring spirit. It was thus that he reached Leipsic, where he sought out Prof. Erdmann who admitted him to his laboratory. At this time he had not learned to observe. Endowed with a vivid imagination, he allowed himself to be led by a preconceived idea and gave to it a degree of confidence not always warranted by facts. His first works especially bear this mark. They drew upon him severe criticism, and were the cause of intemperate discussions not yet forgotten. These discussions more than once occasioned for him wakeful nights; but they contributed on the one hand to call attention to him, and on the other to make him doubt his speculations, and submit them to experiment before publication. From this time he changed his methods of work and devoted to the laboratory a part of the time he had passed in his study. In this he was greatly aided by Laurent, who, in adopting his opinions on many questions, especially the constitution of draconic acid, and by admitting his equivalents for it, gave to Gerhardt and his theories an importance which before had been refused.

His first work was a note upon silicates, soon followed by another on the essential oil of elecampane (Helenium), containing extraordinary numbers and formulæ, which afterwards he himself corrected.

After he had learned with Bacon that in experimental science, experiment before everything else must decide, his synthetic and sagacious mind was of great service to him. At the occurrence of some ideas on the constitution of *Salicine* he made experiments which led him to a new method of preparing salicylic acid. He also prepared the nitrosalicylic acid and was not slow to suspect its identity with the indigotic acid, which he had never seen. He prepared the latter and ascertained in fact the correctness of his opinion.

Gerhardt was then twenty-five years old. Having quarreled with his father when he decided to devote himself to science by entering Erdmann's laboratory, he was soon obliged to leave for want of means. He returned to Strasbourg, but not having been able to overcome his father's will, and not being willing to pursue a career to which he was more and more opposed, he cut everything short by becoming a soldier. At nineteen years we see him enrolled in a regiment of lancers, doing military duty, grooming his horse, and mounting guard, while thinking still of chemistry and vaguely catching glimpses of relations which had not before been suspected.

But if being a traveling clerk was opposed to scientific speculation, military service was still more so. His father yielded, bought him off, persuaded that after this lesson the son would be docile. But the vocation of the young man was determined, and the promises he had made and the remonstrances of his father were alike powerless before his irresistible passion for science.

The year after Gerhardt was at Paris, having previously passed some months in Liebig's laboratory at Giessen. Without position or money

he lived by making translations or giving lessons in chemistry. He made the acquaintance of Cahours, and undertook with him researches upon the essential oil of *cumin*, and made by means of it a series parallel to that which Laurent had established with benzoic acid. Noticed by Thenard, who, as far as he could, used his influence to give the means of work to young men who appeared to him to love science, he was soon in a condition to continue freely his labors. Thenard did for him in fact what he had done for so many others. He committed to him the chair of chemistry in the Faculty of Sciences at Montpellier, on the 16th of April, 1841. Gerhardt delivered as his inaugural address a remarkable memoir on chemical equivalents, advancing new equivalents, which, rejected then by all the chemists of his time, led him to fruitful results, among which may be mentioned the theory of homologous bodies, first brought out in his *Précis de Chimie Organique.*

As if he foresaw his premature end, Gerhardt was in haste to produce. His communications to the Academy succeeded one another with a rapidity that justified the distrust generally accorded to the results which he announced. Thus did he rudely expiate the errors of this first phase of his scientific career. But as has been said, he was not at this time the master of his imagination. However, all was not error in his assertions. Gerhardt had seen the truth in spite of his superficial and imperfect experiments,—as shown by the transformation of the essential oil of mustard into that of garlic, and the formation of nitrous ether by brucine and nitric acid. Much has been said and written against this last announcement made known in 1844 ; and about *quinoline* too, a volative and oily alkali found by Gerhardt in the products of the decomposition of quinine. Nevertheless this alkaloid exists : but as it was the first time that a product of the decomposition of alkaloids had been studied, its composition appeared more than doubtful, its properties having been imperfectly investigated. Emboldened by his imagination, and urged on by other ideas, Gerhardt threw open the subject to discussion by publishing it in the *Comptes Rendus* of the Academy of Sciences.

His inconsiderate publication on the formation of nitrous ether by means of *brucine* is due to a similar cause. Laurent was the first who did not absolutely reject the conclusions of this research,* so severely criticised by Liebig.†

It is true that at that time nothing allowed him to comprehend, much less to explain, the presence of the radical alcohol in an alkaloid, and before admitting such an opinion it was right to demand some well observed facts. The facts were sought for in vain in the memoir. They were afterwards discovered by the pupils of Liebig, who occupied themselves with the subject on the invitation of their master; their explanation is found in the works of Wurtz and Hofmann, *Sur la formation des bases organiques amminées.*

Gerhardt remained at Montpellier till 1848. His instructions had been attended with moderate success. Hoping for a chair at Paris he went to the capital at the end of March. This time the expectations of the chemist were entirely at fault. He was deceived, but on the other

* M. Liebig et la Chimie, Revue scientifique et industrielle, 1846.
† M. Gerhardt et la Chimie, ibid., 1845.

hand he met Laurent, who during these interviews was appointed to the Mint. Laurent was then arranging a small laboratory and with characteristic generosity he offered his hospitality to his friend.

Gerhardt now passed all his time in the damp and dark cellar where Laurent laid the foundation of the disease which prematurely caused his end. A great number of works published in common owe their origin to this partnership. Discussion was the order of the day in that laboratory and it continued in the presence of a third person, competent or not. The crucible had not time to grow cold, but was always on the fire.

Toward the end of Laurent's life, Gerhardt, to increase his resources, opened a laboratory for pupils. This enterprize had but little success, both because of the method which was there pursued and because of the inattention of the instructor, who seldom appeared, less from want of inclination than from lack of time. For, seeking security where there was none, he had undertaken all sorts of writing, and dissipated his strength in all sorts of directions, solely to be able to meet his engagements. Thus, he agreed to contribute to Bouillet's *Dictionaire des Lettres des Sciences et des Arts* all that pertained to science, although he knew nothing but Chemistry! He undertook translations, wrote short treatises, and thus robbed science forcibly of precious time which he could have rendered more productive if his means of support had been guarantied. He remained in this precarious condition till February, 1855, when by the intervention of Dumas he was called to the Faculty of Sciences at Srasbourg. One idea governed him from this time,—to prepare a treatise on organic chemistry in continuation of Berzelius, a work he had commenced four years before under the following circumstances.

One of his friends, a chemist and somewhat of a physicist, who was also then in adversity, had just made an invention which was full of promise, and the success of which would secure him ease if not a fortune. This friend awaited only the realization of his hopes in order to buy up the whole edition of the *Traité de Chimie*, (which in Gerhardt's opinion had need of recasting,) and thus to withdraw it from trade so as to force its author to make a new edition. He informed Gerhardt of his project and invited him to enter upon the work without delay. The latter gave no occasion to repeat the proposal, but went to work with his accustomed eagerness, and in two months his work was almost done.

But before his friend was able to carry out the plan, an opportunity offered for completing the French edition of Berzelius. The idea of introducing his views and opinions into the work of his illustrious and terrible adversary pleased Gerhardt as much as the certainty of having for readers in France all the readers of the distinguished Swedish chemist. He put himself to the work, making a concession, however, on the subject of his notation. It consisted in marking with a bar the symbol of which the equivalent is represented by two atoms, such as ammonia which Berzelius represented by Az^2H^6 and which the partisans of equivalents wrote AzH^3. To reconcile such different notations Gerhardt wrote the A and H with a bar across, a manner of writing formulas already employed by Berzelius and which serves to show that Az and H^3 should be taken twice to represent one equivalent of azote and three of hydrogen.

Aside from this slight concession, Gerhardt presented his views and theories with all the rigidity of system. Knowing the great service which his ideas had already rendered to science, he had the right to believe that their part was not yet ended; only like all innovators he carried his conviction even to fanaticism. Less benevolent than Laurent, he did not allow his pupils to hold other views than his own, but like him he seized every opportunity to make known his ideas; all situations were good for this; but in talking chemistry he did not ask if his interlocutor understood chemistry, or if he knew of what it treated. If his partner was competent he often gave answers which provoked reflection and thus brought light on points before obscure,—the contradictor thus becoming, in the language of another, "*accoucheur d'idées*," and rendering a service which he rarely suspected.

It was thus that the idea of *paramorphism* came to Laurent at the end of a conversation with a friend of his, a chemist and crystallographer, who designated a particular case of isomorphism by the name of *isomorphisme de transition*. It was thus also that the idea of the chemical series came to Gerhardt at the conclusion of a conversation with a socialist, and with the botanist Dunal, whom science has just lost.

Nothing gave a premonition of the sudden death of Gerhardt. Five days before his death he had bathed in the Rhine and had corrected the last proof of his treatise on Organic Chemistry. Several times, however, he happened to say, "Provided I shall be able to finish the publication of my treatise." Having been on the evening of the 15th of August with Mad. Gerhardt to see the illumination of the Cathedral of Strasbourg, he said "I shall never see it thus illuminated again," he returned immediately, complained of cholic, and three days afterward he could say like Laurent, "I shall never see the triumph of my ideas."

Gerhardt had the good fortune to perceive the necessity of having an organ where he might make known his ideas. In 1845, he established the *Comptes Rendus des travaux de Chimie exécutés aux laboratoires de Bordeaux et de Montpellier*. In these *Comptes Rendus* he also analyzed from his own stand point, works published by other chemists and was not slow to raise against himself a general outcry from the freedom of his opinions and the boldness of his criticisms, which, it should be remembered, never went beyond the demands of courtesy and always left persons out of view in the question. It is known that his adversaries were less generous, and the epithets of deceitful man, and highway robber were lavished on him and Laurent. Justice has for a long time been done him, and those who uttered this injurious language were the first to regret it, and to agree that there was some good in the ideas originally condemned.

His *Comptes Rendus des travaux de Chimie*, a journal full of originality which will one day command a price like that of the *Memoirs d'Arcueil*, had a select and discriminating circle of readers. Notwithstanding this, the publisher, did not pay his expenses. Gerhardt then offered his aid to the *Journal de Pharmacie et de Chimie*, an important publication which worthily maintained its rank by the side of the *Annales de Chimie et de Physique*, and after 1846 he prepared for that journal, *la Revue des travaux Chimiques publiés à l'étranger*, a review which has twice since changed its editor.

In the hope of increasing his resources, by assuming in addition to a chair in the faculty of Sciences a chair of Chemistry in the School of Pharmacy at Montpellier, he entered the School of Pharmacy at Strasbourg in 1844. He there sustained a thesis, entitled *Sur la generation d'Ether*, in which he considered ether as a pyrogenous substance. This discussion caused a sensation, because of the difficulties raised by the Professor of Chemistry, then also Director of the School. He was a chemist little known, and jealous especially of young men who promised to distinguish themselves in science. But the objections which he raised against Gerhardt did not prevent his reception as a pharmaceutist, nor his succession soon after to the same chair of Chemistry. Gerhardt occupied this post at the time of his death.

This notice hastily written makes no pretension to being a biography of Gerhardt, and still less a critical examination of his works. Like the notice of Laurent written soon after his death in April, 1853, it is designed to aid whoever will write the history of these remarkable men. The moment of undertaking that task is not yet come, for no one can now determine the part which these two innovators have taken in the scientific movement of modern times, and the influence which they will have exerted on the progress of chemistry.

Submarine Telegraph.—According to late accounts, the telegraphic cable which was to connect Algeria and France had been laid up to a few miles from Galite, having traversed depths of 200 metres and more. A supplementary cable had been ordered at London, but while waiting for this supplement which was all that was necessary to the completion of this great work, a break was discovered in the cable already submerged—at a distance of 500 fathoms from the ship employed in the work.

Society of Acclimation—Results at the Central Nursery of Algiers.—We have already said that the government had established at Algiers a nursery for the purpose of experimenting upon the acclimation of vegetable productions. M. Hardy, the Director of this establishment, gives the following information:

Caoutchouc.—Three specimens of *Ficus elastica* from the coast of Coromandel, were planted about 12 years since. They are near 10 metres high and their trunks are 80 centimeters in circumference at one metre from the soil. Their branches which extend horizontally, occupy very great space, and from their branches spring new roots which planted, add greatly to the vigor of the tree. This mode of cultivation demands great areas.

M. Hardy has drawn from these trees a moderate quantity of caoutchouc, which was placed in the Universal Exposition of 1855. He hopes for much greater results, as a number of questions relative to these trees yet remain unsettled.

Wax and Tallow.—M. Hardy has not yet succeeded in acclimating plants of the family of *Guttiferæ*. The *Myrica sebifera*, of Louisiana, prospers, but needs marshy soils.

The *Croton Sebiferum* from China, is very prosperous. It is now six years old and commences to bear fruit. The wax palm, *Ceroxylon andicola*, which grows in the Andes gives hopes. The sugar-sorghe secretes at the surface of its stalks of full maturity, a white resinous powder, from

which candles could be made. A hectar of Sorgho gives more than 100 kilograms of this substance.

The trials made to acclimatize the trees of Gutta Percha have not yet been successful. The same is true of the trees of quinina.

Artesian Wells in Paris.—The construction of Artesian wells in the *Bois de Boulogne*, spoken of in a previous number, is progressing with activity. The boring is now carried down 435 metres, and it is hoped to reach by the end of October a depth from which the water will rise.

No difference has yet been experienced in the nature and thickness of the earth pierced by the auger and that which it was necessary to pierce for the establishment of the Artesian well of Grenelle. It is known that the section of the wells at Passy will be 60 centimeters in diameter throughout its depth, and that it will be sunk at least 25 metres in the humid bed of greensand that lies at an average of 550 metres below the plain of Passy.

The boring is much easier at Passy than at Grenelle. The construction of the latter well continued through seven years; and it was several times abandoned because of discouragement. Arago and Hèricart de Thury by urgency secured the resuming of the works which it is well known were crowned with success, a jet of water having been obtained rising 28 metres above the soil.

When the well at Passy shall be finished its sides will be supported by an oak lining forming a *tube de retenue;* such tubes have previously been of iron.

Unexpected accidents sometimes impede work. For some months the boring machine was occupied, at a depth of 366 meters in a mass of gray sandstone, where the resistance was so great that a part of the instrument weighing 50 kilogrammes rested in the rock. It was proposed to extricate it by means of powerful electro-magnets, but it appears that the German laborers employed in the work opposed this experiment so decidedly that there was no other resource than to break the piece of iron by the chisel. Thirty-three days were spent on this tedious task. The pieces of apparatus are now arranged in order among the specimens of strata successively traversed, making a very interesting collection.

Charts of the Ecliptic.—Some time ago, on the proposition of Lalande, the Academy of Berlin undertook the first chart of the ecliptic. Their earliest reward was the discovery of the fifth small planet by Mr. Hencke of Driessen. Toward 1847, Mr. Valz of Marseilles developed a plan, the execution of which would lead to the detection of all the planets in the zodiac. Mr. Chacornac, then studying astronomy with Mr. Valz, immediately applied himself to these new charts of the ecliptic. Two months afterward, Sept. 1852, he discovered a new planet. England and Ireland engaged in the same direction; Hind and Cooper were soon to distance the French astronomers. However Chacornac, who meanwhile had been attached to the Observatory of Paris, continued his charts. They have just been published at the expense of that observatory. They will be more complete than the former, and quadruple the dimensions of those of Berlin, the scale being 50 millimetres for each celestial degree.

These charts contain all the stars of the 12th magnitude and many of the 13th magnitude: they extend above and below the ecliptic to 5° of

declination. Their form is square. The number of stars inscribed on them already surpasses 125,000.

Aërostats.—The Academy of Sciences in Dijon having asked of that in Paris aid and money for an aërostatic ascension *à ballon captif* which it proposes to cause, a discussion arose in the Academy of Paris in regard to the utility of such ascensions were for scientific purposes. Marshal Vaillant, Minister of War, mentioned on that occasion the trials made in the Spring of 1855 at Vincennes under the direction of Artillery, Engineering and Marine officers. The object was to ascertain if it was possible to maintain a balloon 5 or 600 meters above a fortified town, and if so, to cause incendiary or fulminating balls to fall. Nothing was successful. The commission made two balloons, spent much money and gave up every thing. According to Vaillant, the force of a wind even moderate will always be enough to drive to the earth a captive balloon.

Biot on the contrary defended ascensions *à ballon captif*, having a scientific object. If the descent of the balloon is dangerous above a place of war it is otherwise in an open plain.

Biot who made in 1803 with Gay Lussac a celebrated ascension recalled the many and fruitful experiments made by the school of *aërostiers* founded under the first Republic and which rendered great service in the sieges of Charleroi, and Fleurus.

Jomard, the geographer, who attended this school stated that he had made and witnessed since 1797 a great number of ascensions *à ballon captif* and that Col. Coutelle, sub-director of the school of *aërostiers* never doubted the utility of such ascensions when well directed, which may not have been the case at Vincennes.

Correspondence of Jerome Nicklès, dated Oct. 30, 1856.

On Perfumes.—A new branch of industry is on the point of being brought out in Algeria, which will not fail to be introduced into other countries. This industry is that of Perfumes. A laboratory process has given origin to it. We have spoken several times of the experiments of Millon on wheat. On treating wheat or its farina with ether, some fatty or waxy matters are dissolved, which are more or less colored, and almost always have a strong odor; they are identical with the extract from the grain taken in mass. This aromatic principle is very persistent and may be distinguished still in the fatty matter after several years, though disappearing at the moment the fat becomes rancid.

This fact led to the experiments of which I speak, which had in view the extraction of the aromatic principle of flowers and of some plants peculiar to Algeria.

To avoid the alterations which flowers undergo on drying or distillation, Mr. Millon separates the aromatic part by dissolving it in a very volatile liquid which is afterwards expelled by distillation. With such a solvent, the distillation is attended with no inconvenience, for it may be performed at a low temperature; this chemist finds that the perfume undergoes alteration whenever a temperature is applied above that of the surrounding atmosphere. In some parts of northern Africa, the thermometer reaches +70° C.; he then employs with success the volatile solvents, such as sulphuret of carbon, ether, chloroform, wood spirit,

the point of ebullition in which is below this temperature. He has even succeeded with alcohol, whose point of ebullition is above 70°.

The solvents which succeed best are ether and sulphuret of carbon. The flowers are put into the apparatus, and the ether then poured on so as to cover it. In 10 or 15 minutes the liquid is run off and a new quantity of ether introduced to wash out what is left; this remains as long as the first. The ether dissolves all the perfume and deposits it again on distillation in the form of a variously colored residue, sometimes solid, sometimes oily or semi-fluid yet becoming solid after some time. This residue, when obtained in a thin layer, is fused by the solar heat or an equivalent temperature, and resoftened frequently until it exhales no longer the odor of the solvent.

The solvent, ether or sulphuret of carbon, should have been previously purified with the greatest care. That derived from the distillation may be used indefinitely, provided it is for the same flower and apparatus. Properly managed there is but very little loss of the solvent and the distillation is rapidly performed, much more rapidly and with a larger amount of leaves and flowers than by the ordinary method of distillation. But the gathering of the flowers should be done at the proper time of day for each flower. Thus the Carnation gives off its perfume after an exposure of two or three hours to the sun. Roses, on the contrary, should be collected in the morning as soon as well open; the Jessamine before sunrise.

In the distillation, as hitherto carried on, all the modifications of the flowers are mixed in one and the same essence, which corresponds to no one of them, the better portion partly correcting the rest. But with the Millon process, the slightest alteration is apparent in the perfume, and in order to obtain the freshness and delicacy of the flowers, it is necessary to have them fresh and sweet. The perfection of the flowers determines the perfection of the perfume.

At first, Millon operated by shutting out the contact of the air. But now he favors its presence, for he has found that the perfume, instead of dissipating rapidly like the essences, has great fixedness. It is only through contact with other principles of the plant that it undergoes alteration. Once isolated it is beyond their influence and experiences no further change. Millon thus for several years has kept perfumes at the bottom of open tubes or capsules open to the air without sensible alteration; and according to him, this fixedness or resistance to atmospheric change is a fundamental characteristic of perfumes.

It has not been possible yet to submit the perfumes to elementary analysis, the flowers furnishing so little of it; a kilogram giving only a few milligrams of the aromatic principle.

The residue of the operation by ether or sulphuret of carbon contains wax and fatty and coloring matters, and it is very difficult to separate the aromatic principle from them. Alcohol answers best for this purpose. It does not dissolve the waxy part, while it removes completely the odor. Operating with alcohol on a grain of the residue, the perfume is taken up with a little oil and the coloring matter, and the aromatic residue will have lost in the process only a few hundredths of its weight.

The perfume is almost indefinitely diffusible in the air, showing its presence by its odor, without any sensible loss of weight. It is equally diffusible in *distilled* water, when some drops of an alcoholic solution are poured into it; but in ordinary water the odor is dissipated, showing its easy alterability with reagents.

The facility with which these perfumes dissolve in alcohol, oils and fats, shows the ways in which it may be industrially employed. The essential point is that the small quantity of product afforded by the flower represents exactly the amount of perfume; and a gram of residue proceeding from a kilogram of flowers aromatises to the same degree fat or oil, and under a volume a thousand times less produces the same effects.

The process, then, takes the volatilisable part of the flower, concentrates and preserves it, and puts it up for transfer, without loss, to the perfumery shops, where the final preparations are made. Moreover, the work of incorporating the perfume of the flowers with the fats and oils, to-day so costly and so incomplete, will be replaced by a simple mixing or solution which may be done at any convenient time or place. It is for perfumery, a new art of extreme simplicity.

The interest which attaches in France to these researches of M. Millon will be understood when it is noted that the exports of perfumery from France amount to more than thirty millions of francs per year.

Manufacture of Aluminium.—We have more than once spoken of the efforts employed to render the preparation of aluminium an industrial operation. Dumas has just announced to the Academy that this problem is now solved. He has stated that the manufacture is actually carried on by workmen in a small shop in the Faubourg St. Jacques, at Paris, connected with a manufacture of chemical products. The methods have been contrived by H. St. Claire Deville and Morin. They differ little in general from those originally employed. It is necessary always to decompose the chlorid of aluminium, and decompose it by sodium, in order to obtain the aluminium. But the processes for the sodium and chlorid of aluminium have been simplified. The sodium process has already been described by us, and we now mention the improvements in that for the chlorid.

This chlorid is now made by the direct use of kaolin or even of clay —that of Dreux for example. But this is not all. The chlorid was difficult to manage in a large way because, after having been formed in vapor, it was often condensed in snowy crystals, rendering it necessary to collect it in chambers and detach it mechanically from the surfaces it coated. There was, *first*, a loss of the chlorid, the condensation being incomplete; *second*, danger for the workmen exposed to the respiration of the vapors; *third*, an enhancement of cost from the interruptions in the operations.

The improvement consists in submitting to a current of chlorine—not longer a mixture of alumina and charcoal, but—a mixture of alumina, charcoal and chlorid of sodium; this affords a double chlorid of aluminium and sodium which is volatile and liquifiable, running like water and becoming solid with cold. The preparation goes on uninterruptedly, proceeding with simplicity and regularity and exacting no other care

than what is necessary for the production of the chlorid, the renewal of the preparation for decomposition, and the substitution, as soon as cooled, of earthen pots, in which cakes form from the double chlorid that flows in in a continued stream.

The chlorid is decomposed in a reverberatory furnace, into which, mixed with bits of sodium, it is introduced. The reaction of the two substances takes place after a few moments, but so quietly that it may be done on a large scale without danger. It leaves the aluminium in plates, globules, or a powder. It is separated from the common salt either mechanically or by means of water.

Dumas asserts that the cost of making sodium is at the most 7 francs a kilogram, and that its manufacture is easier than that of phosphorus and also as simple as that of zinc.

By acting on a mixture of carbonate of soda, carbon and chalk, the reaction is so complete that the result agrees with calculation, and so easy that we may substitute for the iron bottle commonly used, luted copper tubes.

In the manufacture of sodium the carbon is now replaced by coal. Deville uses a coal which burns with considerable flame. It is important that the mixture should be well dried before subjected to decomposition. The proportions used are as follows:

Carbonate of soda, - - - - - -	30 kil.
Coal, - - - - - - - - -	13 "
Chalk, - - - - - - - -	5 "

The soda ought to be from the crystallized carbonate; the soda of the shops gives bad results without Deville's knowing precisely why.

Origin of Urea in the Animal Economy.—Dumas has announced with much enthusiasm the confirmation of his views already old respecting the origin of urea in the animal economy, viz., that the urea proceeds from the albuminoid substances destroyed in the blood by an oxydating process. This is now established by M. Bechamp, Professor at the School of Pharmacy of Strasbourg, who has succeeded in changing albumine fibrine and gluten into urea by a slow combination produced by means of a solution of permanganate of potash at the temperature of about 80° C. The following is the process:

Ten grams of aluminum are dissolved in 300 grams of water; and to this by degrees 75 grams of permanganate of potash are added. The reduction, which is at first very active, soon ceases. It is then heated to 40° C. in a water bath, and from time to time saturated with sulphuric acid, yet so as to leave it still a little alkaline. When the discoloration is completed, it is filtered, and exactly saturated with dilute sulphuric acid. The solution, now perfectly limpid, is evaporated in a water bath; and when reduced to a small volume, an excess of concentrated alcohol is added; it deposits some sulphate of potash and sulphate of ammonia. The alcoholic solution is evaporated in its turn to the consistence of honey and treated with hot absolute alcohol which dissolves the urea.

Whilst M. Bechamp was bringing out this transformation, a physiologist, M. Picard, made, also at Strasbourg, some observations bearing on the subject, having reference to the presence of urea in the blood and its diffusion through the system.

It is known that according to MM. Dumas and Prévost, urea shows itself in the blood of animals after the removal of the kidneys, and that they conclude from the fact that the kidneys remove the urea, while not producing it.

M. Picard has completed the demonstratation. He has compared, as regards the presence of urea, the arterial and venous blood by precipitating the urea with nitrate of mercury. The renal artery of a dog afforded 0·0365 per cent of urea, and the renal vein only 0·0186 per cent. In studying the question with reference to man, he has observed that the arterial blood which passes into the kidneys leave there about 28 grams of urea; while the quantity of urea in the urine of the subjects submitted to experiment, varied between 27 and 28 grams for the 24 hours. This proves that the kidneys remove but do not make urea, as announced thirty-five years since by Prévost and Dumas.

Electricity. Theory of the voltaic pile.—During the thirty years in which the theory of the pile has been under discussion, research has favored apparently at one time the *chemical*, and at another the *contact* theory. We are able now, as we believe, to announce that the discussion is closed. The chemical theory has definitely triumphed. As explained in the Traité d'Electricité Théorique et Pratique of De la Rive, this theory meets all difficulties and proves that if contact is often necessary for exciting electricity, it is not that which produces it; chemical actions are always the source. This learned physicist demonstrates that all chemical action causes a disengagement of electricity, whilst not a single experiment can be cited in which electricity is produced simply by contact.

He reviews and explains all the alleged facts in favor of the theory of contact. He thus shows up the objection so often urged, that in order to displace by iron the copper of the sulphate of copper it is necessary to put the iron in contact with the saline solution, and that the chemical action begins only after the iron is covered with copper and when it has thus formed a voltaic couple. De la Rive first proves that in this experiment there is a voltaic couple which precedes that formed by the iron and by the displaced copper; this couple is formed by the iron and the oxyd of iron adhering to its surface or by iron and carbon or some other foreign body; for by using iron chemically pure and a surface perfectly clean, no precipitation of copper is obtained.

No physicist is better fitted than De la Rive to undertake the delicate task of giving a theory of the pile. His studies as well as his discoveries lead him in this direction.

De la Rive was the first who recognized the important fact that zinc chemically pure is not attacked by hydrated sulphuric acid; that two metals on which pure nitric acid, for example, has no action, such as gold or platinum, give not the slightest trace of a current when put to the extremity of a galvanometer and plunged into the nitric acid; that on the contrary, they produce an instantaneous current when a drop of chlorohydric acid is added. The theory of contact has never yet explained this fact.

The third volume of his work is reserved for the applications of electricity. The important part which De la Rive took in the invention of

galvanoplasty is well known, and we may expect to see this branch of industry illustrated in his work with new views and a precision and exactness, of which the first two volumes have given us such fine examples.

Galvano-caustic.—We have already had occasion to speak of the experiments made towards applying in medicine and surgery, the heating effects which are suddenly produced by means of the pile. This question, besides others, was the occasion of some trials by Mr. Amussat. Recently a surgeon, Mr. Middledorp, brought before the Academy a series of instruments and of surgical apparatus, calculated for the employ of electricity, and especially for using the heating effects of the pile in behalf of suffering humanity. The author designates under the name of *galvano-caustic*, the series of operations executed by this means.

The source of heat employed is a Grove's pile. The instruments are, 1, different cauterizers; 2, a galvanic seton; 3, a "porte ligature" for penetrating where there is access to no other cutting instrument, and combining at once the advantages of an incision, ligature and cauterization. A commutator fitted to the conducting wires, serves to excite or suspend the action.

We have already spoken of the advantages of this method of operation. While avoiding hemorrhage from small vessels, it is adapted to produce energetic action, very precisely limited, and to penetrate into regions inaccessible to the bistoury and cautery. By means of the electric heat directed by appropriate instruments, we may cut, make incisions or cauterizations large or small, arrest hemorrhages, provoke the inflammation of tissues, the coagulation of the blood, etc. Moreover, being introduced cold, the galvano-caustic instruments excite no fear; once in place a movement of the finger will develop the requisite temperature.

Crystallogeny.—It is a long time since it was observed by Mr. Marloye, a skilful constructor of acoustic instruments, that when the angle of a crystal is removed with a knife and the surface polished, the crystal, if left for a time in the solution, completes itself again. This fact has been called to remembrance by an observation of Dr. Marbach of Breslau, who adds to it a supplementary fact of interest. The chlorate of soda crystallizes in the cubic system and its solution in water has no action on polarized light. But when this solution crystallizes, it produces crystals which have a rotatory power like rock crystal, some turning to the right and others to the left; and if the crystals of the same kind are separated from the other and dissolved anew, not only the solution has no rotatory power, but also, if left to crystallize, it affords crystals of both kinds, although only those of one kind were used.

This peculiarity of the crystals is connected with the fact that they are hemihedral, and have the reverse hemihedrism corresponding to each kind of rotation. Still the hemihedral planes are of rare occurrence and are produced only when operating on a large scale. But Mr. Marbach has found a means of rendering them distinctly hemihedral; as done by Mr. Marloye, he cuts the angles and edges of the chlorate of soda and then puts the crystal thus mutilated into a concentrated solution of the same salt; the crystal forms new faces which present the right and left hemihedrism.

Artificial milk.—For some time a liquid has been prepared which is said to have so far the qualities of milk that it is called artificial milk or

"lait-viande." It is prepared as follows. Into a Papin's digester three kilograms of fresh pounded bones are put and one kilogram of meat, with five or six times as much of water. The top is hermetically closed: double sides surround it, and in the cavity between, a current of steam circulates which raises the temperature of the digester up to 140° F. At the end of forty minutes after reaching this temperature, a stop-cock with a small orifice is opened which lets out a vapor having the odor of broth; but some seconds after, there issues a white liquid which is nothing but the *artificial milk*. After this milk has passed out, the digester contains only the meat, the boiled bones, and a soup of inferior quality. The artificial milk resembles milk in color, consistence, odor, and even taste. But in composition it is different; for it is only an emulsion produced by the fat mixed with the water by means of the gelatine. Although the name artificial milk is not proper, it has some nutritious qualities, and for this reason it is now under trial at the hospitals of Paris.

Universal Exhibition of Photography.—In a recent exhibition in Belgium for the encouragement of the useful arts, a large part was devoted to photography. Many of the objects had already been exhibited at Paris; but there were some novelties. Heliographic engraving, brought out but yesterday, has already attained great perfection, judging from the specimens exhibited by Mr. Riffaut as well as Mr. Nègre. Fine photolithographic views were exhibited by Mr. Poiterin and Dr. Harless, the latter after a process of his own invention.

Among the photographic objectives on exhibition, those of Mr. Jamin, of Paris, deserve special mention. There was one of 14 inches which would answer for obtaining pictures 1·6 meters to 1·2. There was a portrait taken on a glass 60 centimeters by 80, with a double objective of 22 centimeters diameter. The objective was furnished with a conical centralizer.

Bibliography.—*Traité d'Electricité théorique et appliquée*, par A. De la Rive. Vol. II, 836 page. Paris: Baillière.—In announcing the 1st volume of this remarkable work we stated that it would consist of two volumes. But the author has given so much greater extension to some subjects, especially to the theory of the pile, already alluded to, that he has found it necessary to add a third to include the applications of electricity. The present volume treats of the *Preparation of Electricity; its calorific and luminous effects; the chemical effects of dynamical electricity; the physiological effects of dynamical electricity on organized bodies.* An important chapter is devoted to the sources of electricity, such as heat, mechanical actions, and finally the chemical actions to which De la Rive has himself contributed so largely. The mathematical branch of the subject is treated in a chapter by itself, and a note is devoted to the mathematical theory of the pile. The 3d volume will appear next year.

Elémens de Cosmographie, par Costambert. 1 vol. in 12mo, with an atlas. Paris: Hachette.

Médecine Domestique, par le Dr. Beaugrand. 1 vol. in 12mo. Paris: Hachette.—Dr. Beaugrand is a distinguished physician of Paris.

La Telegraphie électrique, par V. Bois. A pamphlet in 12mo.—The object of this small work, which has passed rapidly to a 2nd edition, is to popularise the subject of the Telegraph.

Les Abeilles et l'Apiculture, par M. de Frarière. 1 vol. in 12mo. Paris. Hachette.—The author is a great lover of bees. He has made them the subject of much study, and gives in this work the results of his useful labors.

Compositions de Mathematique et de Physique, par M. Jouve. 1 vol. in 8vo. Paris: Hachette.—This work contains a large number of problems in Physics and Mathematics, intended for the education of the young who have science, commerce or the arts in view. It is highly appreciated by masters and students, connected with the educational institutions of Paris, and France generally.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On the wave length of the most refrangible rays and of those which act chemically upon iodid of silver.*—Eisenlohr has communicated some interesting additional facts with respect to the diffraction of the more refrangible rays of light. The author's earlier results have already been mentioned in this Journal: those now communicated were obtained by photographic impressions of diffraction spectra taken upon a film of collodium-iodid of silver placed at a distance of 5·58 metres from the objective of the lattice. The author measured the distances from the middle of the image of the slit to the borders of the different lateral spectra and thence determined the wave lengths of the corresponding rays by the formula $u\lambda = e \sin \psi$.

By a comparison of the wave lengths thus obtained it appears that we may assume, *that the limit of the invisible most refrangible rays is at the same time one limit of the rays which act chemically upon iodid of silver, and that the other limit of the less refrangible rays acting upon the iodid is as definite and sharp as the first.* Both fall between the wave lengths 0·000354^{mm} and 0·000433^{mm}. The author remarks that this sharp limitation might also have been inferred from a communication of Müller (in Pogg. Ann. vol. 97, p. 137) who projected prismatic spectra upon photographic paper. It is certainly true for rays of sunlight acting upon iodid of silver, but the author is not prepared to say that it is true for all kinds of light. According to Crookes the chemical rays in gas light fall in a different part of the spectrum from those of sunlight, also the rays between F and G which have no action on iodid of silver readily decompose the bromid. The author proposes to investigate this subject by means of the diffraction spectrum. The intensity of the chemical action upon iodid of silver is greatest and nearly uniform between the lines G and H or the wave lengths 396 and 429 millionths of a millimetre. It diminishes slowly from 396 to 354, but rapidly from 429 to 433, which probably arises from the great number of the dark lines which Stokes has denoted by *l*, *m* and *n*. The author further states, that from analogous experiments there appears to exist just as definite a limit for the thermic action of light.—*Pogg. Ann.*, xcix, 159, *August*, 1856.

2. *On Tantalum and its compounds with chlorine and bromine.*—Rose has communicated an interesting memoir upon this most difficult subject to which he has devoted so much time and talent. The author remarks that tantalum has hitherto been found with certainty only in two

minerals, namely, tantalite and yttro-tantalite, the tantalite occurring in Finland, Sweden and France, yttro-tantalite at Ytterby in Sweden. The tantalic acid employed was obtained partly from the Finland mineral and partly from materials left by Berzelius. The acid was prepared from tantalite by fusing the finely pulverized mineral in a platinum crucible with bisulphate of potash: the fused mass was washed with cold water and the undissolved acid fused with three times its weight of a mixture of carbonate of soda and sulphur. Water then removed all the tin as sulphid. The tantalic acid was washed with water containing sulphid of ammonium, and then digested and boiled with chlorhydric acid to remove the sulphid of iron, and finally completely washed. An acid obtained by decomposing the chlorid with water was also sometimes employed, but this differs in many particulars from that obtained as above. The author obtained metallic tantalum by decomposing the double fluorid of tantalum and sodium by metallic sodium. As thus obtained, tantalum is a black powder which is a good conductor of electricity, though mixed with much acid tantalate of soda. When ignited in the air it oxydizes to tantalic acid with evolution of light and heat. Nitric, chlorhydric and nitromuriatic acids have no action upon it even by long boiling. Fluohydric acid slowly dissolves the metal with evolution of hydrogen. A mixture of nitric and fluohydric acid quickly dissolves the metal with evolution of red vapors. The solution when evaporated to dryness and heated, yields a residue of tantalic acid while fluorid of tantalum is given off. Sulphuric acid has no action on the metal, but by long fusion with the bisulphate of potash it is oxydized to tantalic acid. The density of the metal was found to be 10·078, and after ignition in hydrogen 10·776; the metal employed was not, however, pure. The author prepared the chlorid of tantalum by igniting the acid with carbon and passing a current of chlorine over the mixture. The chlorid has a pure yellow color and only looks whitish when it contains aci-chlorid; it must volatilize completely by heating in an atmosphere of chlorine. The color of this chlorid is somewhat brighter than that of chlorid of niobium: it is easily fusible and at about 221° C. becomes a yellow liquid which sublimes to a crystalline sublimate. It begins, however, to volatilize before it melts, namely at 144°. The chlorid gives no double compounds with alkaline chlorids. It is soluble in anhydrous alcohol and is not precipitated from the solution by sulphuric acid even after boiling. When the alcoholic solution is distilled, a syrupy liquid remains which probably consists chiefly of tantalate of ethyl. The determination of the equivalent of tantalum was effected by the analysis of the chlorid which was decomposed by water, and the solution filtered after addition of a little ammonia: the chlorine was then determined in the filtrate by nitrate of silver. In this manner twelve experiments were made, which, however, do not exhibit as close a coincidence as could be desired. The author takes a mean between the result of the first and eleventh analyses, which exhibit a close correspondence between the quantity of oxygen found and that calculated from the amount of chlorine. According to these results the composition of the chlorid and oxyd in parts per cent is

Tantalum,	49·25	Tantalum,	81·14
Chlorine,	50·75	Oxygen,	18·86
	100·00		100·00

These results differ greatly from those obtained by Berzelius. Rose, after much consideration adopts the formula TaO_2 for tantalic acid, which gives in connection with the above results 860·26 (O=100) or 68·82 (O=8) for the equivalent of the metal. The author obtained a bromid of tantalum by passing the vapor of bromine over an ignited mixture of tantalic acid and carbon. The bromid has a yellowish color, and resembles the chlorid; water decomposes it into bromhydric acid and tantalic acid. Rose did not succeed in obtaining a corresponding iodid of tantalum.—*Pogg. Ann.*, xcix, 65, *August*, 1856. W. G.

II. MINERALOGY AND GEOLOGY.

1. *Description of a new Meteoric Iron from Chili, containing Native Lead; and an account of a fall of a large mass of Meteoric Iron at Corrientes in South America;** by R. P. Greg, Esq., (Phil. Mag., July, 1855.)—(1.) A short time since I purchased a mass of meteoric iron weighing upwards of seventeen pounds; its shape was irregular and cup-like, considerably convex or hollowed out on one side, and the external surface more or less covered with small angular and conchoidal projections. It was found by Mr. Greenwood, Reporter of Mineral Properties, on the 26th of February, 1840, on the desert of Tarapaca, eighty miles northeast of Talcahuano, and forty-six miles from Hemalga; and was afterwards analyzed, in 1853, by Mr. George Darlington, of the Museum of Practical Geology, with the following results:—

	a	*b*
Iron,	93·41	93·48
Nickel,	4·62	4·56
Cobalt,	0·36	0·37
Manganese,	0·20	0·18
Phosphurets,	1·21	1·26
Chromium,	*trace*	*trace*
	99·80	99·85

In general composition it therefore closely resembles the majority of meteoric irons hitherto analyzed. I am unable at present to say if it contains *Schreibersite.*

The specific gravity of a slice weighing six ounces, containing, however, cavities and other matter, I found to be about 6·5. For meteoric iron it is perhaps unusually soft; and though it shows no regular *crystalline* or Widmannstättian figures when a polished surface is treated with nitric acid, yet there is apparent a slightly welded or mottled texture, brighter in some parts than in others.

It was not, however, until the iron had been cut up into slices for the purpose of polishing and for exchange, that I discovered it had not a perfectly homogeneous structure, but was in many places more or less honeycombed with cavities, some of which actually contained what appeared to be pure *lead!* In some the lead was not larger than a pellet, and did not fill the entire cavity contained in it; in others the entire cavity was filled with lead, in size equal to a pea. Professor Shepard of

* From the Liverpool Literary and Philosophical Society's Journal.

the United States, who is so well acquainted with meteorites, along with Dr. Heddle and myself, saw some slices of this iron slit in the workshop of Mr. Young the lapidary, at Edinburgh, and we took lead out of the cavities immediately after they left the lathe, so that there could be no deception whatever.

To be quite certain, my friend Dr. Heddle examined some of it, and found it to be chemically pure lead; when the tarnished surface was not scraped off, small quantities of iron and alumina, and mere traces of phosphorus and magnesia were found.

There are also two other substances in some of the cavities of this singular iron, which Dr. Heddle, when he has analyzed, will separately report upon; the one a very hard, grayish-black, semi-metallic mineral; the other yellowish-brown, insoluble in acids, and with an earthy texture.

This is the first authentic instance of the existence of lead in meteoric bodies, and to find it so closely allied with, and buried, as it were, in metallic iron, is not only in itself singular, but difficult to account for. It is, however, probable that the lead was originally held in alloy along with the nickel and cobalt, and on intense heating or partial fusion of the iron mass, "sweated" out into vesicular cavities.

Should this be a correct view, it is a proof of the intense heat to which iron meteoric masses appear to have been subjected at the time of, if not previous to, their reaching the surface of the earth. Indeed meteoric *stones* seem to have been subjected to a much smaller degree of heat while falling, than *iron* masses, if we may judge by appearances, the only sign of fusion in *stones* being quite external, and merely marked by a thin, black, and shining crust.

Iron falls are extremely rare compared with what are called *stone* falls; so much so, indeed, that there are not more than three or four authentic accounts of the fall of *iron* masses, and these not large ones, bearing no comparison to the enormous masses weighing from five to twenty tons, which have been occasionally found on the plains of Mexico and South America. (See Phil. Mag. for Dec. 1854.)

(2.) Should any one be inclined to doubt the fact that such immense masses of iron have, strictly speaking, a meteoric origin, I have added to this paper some particulars of the fall of one in South America in the year 1844, that first appeared in a philosophical journal some years ago, but which, from having a local circulation, has not received the notice which it so eminently deserves.

It is important to notice the state of intense fusion exhibited by the entire mass at the time of falling; Mr. Symond's description is most graphic. The account was given in a letter, read by Mr. Dickinson, from the observer of the phenomenon, a Mr. H. E. Symonds; and the following is an extract:—

"Having been deeply engaged in Argentine politics and wars in 1843 to 1844, I accompanied the Corrientine army in its invasion of the province of Entre Rios. This army returned from that expedition in January, 1844. Our rear, in which I marched, was so continually harrassed by Entrerian skirmishers, that for ten days before we had gained the Corrientine frontier we had no time to sleep or change clothes; but soon after passing this, in Carritas Paso, on the river Mocorita, we placed

a guard in the pass, and deeming ourselves secure, the whole division abandoned itself to the profoundest sleep.

"From this sleep we were all simultaneously awakened at about two o'clock in the morning; and as if actuated by electricity, each individual of our division (about 1400 men) sprung on his feet at the same moment. An aërolite was falling. The light that accompanied it was intense beyond description. It fell in an oblique direction, probably at an angle of about 60° with the earth, and its course was from east to west.

"Its appearance was that of an oblongated sphere of fire, and its tract from the sky was marked by a fiery streak, gradually fading in proportion to the distance from the mass, but as intensely luminous as itself in its immediate vicinity. The noise that accompanied it, though unlike thunder, or anything else that I have heard, was unbroken, exceedingly loud and terrific. Its fall was accompanied by a most sensible movement of the atmosphere, which I thought at first repellent from the falling body, and afterwards it became something of a short whirlwind. At the same time I and my companions all agreed that we had experienced a violent electric shock; but probably this sensation may have been but the effect on our drowsy senses of the indescribably intense light and noise. The spot where it fell was about one hundred yards from the extreme right of our division, and perhaps four hundred from the place where I had been sleeping. Accompanied by our general (Dr. Joaquin Madauaga), I went within ten or twelve yards from it, which was as near as its heat allowed us to approach.

"The mass appeared to be considerably imbedded in the earth, which was so heated that it was quite bubbling around it. Its size above the earth was perhaps a cubic yard, and its shape was somewhat spherical; it was intensely ignited and radiantly light, and in this state it continued until early dawn, when the enemy, having brought his artillery to the pass, forced us to abandon it to continue our march. I may mention, that, at the time of its fall, the sky above us was beautifully clear, and the stars were perhaps more than usually bright; there had been sheet lightning the previous evening.

"I never afterwards had an opportunity of revisiting the Mocorita, for our permanent encampment was thirty-five leagues to the north of that pass, between which and our encampment the country was entirely depopulated by our long war; but as the spot where the aërolite fell was known to many of our subaltern officers, who were frequently sent to observe the frontier of Entre Rios, I have heard them describe it as a 'piedra de fiérro,' *i. e.* a stone of iron; and I once provided one of the most intelligent of them with a hammer in order that he might bring me a sample of it. On his return, he told me it was so excessively hard that the hammer bent, and was broken in unsuccessful attempts to break off a small piece of it."

2. *Hard Guano of Monks Island;* by A. Snowden Piggot, (from a letter to one of the Editors).—I was not a little surprised to read Professor Shepard's views of the hard guanos. There is no trap rock at all about the majority of the beds of this substance. It covers ordinary

"alluvial guano" as the Captain called the common variety brought from the Caribbean Sea. Indeed I have seen nothing that looked to me like trap from any of those islands, I have seen quartz crystals, granite, gneiss and some crystalline rocks that I could not determine among the cargoes. Besides, the constitution of the phosphates proves conclusively that they have not been hardened by fire. I have made another examination of the rock, without the enamelled surface and am fully convinced of the correctness of my views. The following are the results:

The phosphoric acid in this table does not include the small amount existing in the phosphates of iron and magnesia which were estimated separately.

Phosphoric acid,	46·22
Lime,	38·75
Phosphate of iron,	0·35
Phosphate of magnesia,	0·61
Sulphuric acid,	1·96
Chlorine,	*trace*
Organic matter, salts of ammonia, and combined water,	8·95
Hygroscopic water,	2·34
Sand,	0·63
	99·81

If we take from the lime sufficient to combine with the sulphuric acid, we have remaining of that earth 35·95, which when combined with the phosphoric acid and 5·78 parts of water, give a salt of the formula $2CaO, HO, PO_5$.

The small quantity of the phosphates of iron and magnesia in this specimen is worthy of notice. It is a little remarkable, that in these guanos generally, while the entire amount of phosphoric acid varies but slightly, there is a very wide difference in the proportions of the bases with which it is combined.

The "body of the rock" of which the above table expresses the composition, was carefully separated from the white enamel before analysis.

Baltimore, November 11th, 1856.

3. *On Columbian Guano, its composition and character;* by JAMES HIGGINS, M.D., and CHARLES BICKELL, Ph.D.—Columbian guano presents itself in the form of irregular stony lumps, apparently composed of two distinctly different parts, viz: of an *exterior layer*, only a few lines in thickness, and of the "body, the great mass of the rock," directly underlying the exterior part. The former is of a grey or dirty white color, with vitreous lustre, hard but brittle, and therefore easily ground in a mortar; the interior part is of a dark brown mottled color, without any lustre, tough, horny and tenacious as to texture, and with difficulty reduced to powder.

A proper chemical examination of this substance is therefore naturally divided into two separate analyses, that of the exterior layer, and that of the body or great mass of the rock; the latter representing at the same time the composition of the genuine commercial article.

Several carefully conducted analyses resulted in the following composition of the respective parts:

	Exterior layer.	Body of rock.
Lime,	41·76	39·01
Magnesia,	3·10	0·22
Peroxyd of iron,	0·34	0·11
Phosphoric acid,	39·92	43·50
Calcium,	1·34	——
Fluorine,	0·85	——
Chlorine,	0·75	*trace*
Soda,	1·61	——
Sulphuric acid,	2·07	7·08
Water and organic matter,	8·60	10·75
Sand,	0·02	0·02
	100·36	100·69

The relative proportions of the different constituent substances above given, necessarily lead to the following state of their combination:

Exterior layer.

Tri-phosphate of lime,		77·71
containing of lime,	41·76	
" " phosphoric acid,	35·95	
Tri-phosphate of magnesia,		6·77
containing of magnesia,	3·10	
" " phosphoric acid,	3·67	
Phosphate of iron,		0·64
Fluorid of calcium,		1·76
Chlorid of calcium,		1·18
Sulphate of soda,		3·68
Water and organic matter,		8·60
Sand,		0·02
		100·36

Body of the rock.

Common phosphate of lime,		82·48
containing of lime,	34·04	
" " water,	5·44	
" " phosphoric acid,	43·00	
Common phosphate of magnesia,		0·66
containing of magnesia,	0·22	
" " water,	0·05	
" " phosphoric acid,	0·39	
Phosphate of iron,		0·22
Sulphate of lime,		12·05
containing of lime,	4·97	
" " sulphuric acid,	7·08	
Water and organic matter,		5·26
Sand,		0·02
		100·69

It will be seen from the above that in both parts of the rock the phosphoric acid is present in the form of a tribasic phosphate (containing

three atoms of base to one atom of acid), with the difference that in the *exterior layer* the three atoms of base are made up by lime alone, thus forming the so-called *tri-phosphate of lime*, whilst in the *body of the rock* only two of them are lime and the third is water, a combination which is known as *common phosphate of lime*.

In the following we add a few items which may prove the correctness of this view:

(1.) Common phosphate of lime, when exposed to a high degree of temperature, loses its volatile atom of base (the water) and is consequently transformed into a phosphate of two atoms of fixed base, or a pyrophosphate. Tri-phosphate of lime, under the same conditions, remains unaltered. In accordance with this, the hydrochloric acid solution of the natural, as well as of the heated powder of the *exterior layer*, if treated with ammonia, produces a precipitate which is, in both cases, soluble in acetic acid. This is also the case with the natural powder of the *body of the rock;* whilst its heated powder, if treated in the same manner, remains insoluble in acetic acid, as pyrophosphates do. But pyrophosphates can only be produced in this way by common phosphates; therefore, the *body of the rock* is composed of common phosphate of lime, the *exterior layer* of tri-phosphate of lime.

(2.) Common phosphate of lime, when brought into contact with ammonia, is converted into triphosphate of lime, whilst phosphate of ammonia is found in solution. This fact may explain the relation which exists between the *exterior layer* and the *body of the rock*. The latter, we have seen, is composed of common phosphate of lime and some organic matter; on its surface, where it was exposed to rain and to the influence of the atmospheric ingredients (especially ammonia), its organic matter became slowly decomposed, and ammonia evolved from it. The ammonia thus produced by the organic matter of the rock, as well as that portion conveyed to it by the atmosphere, necessarily came in close contact with the common phosphate of lime on the surface of the rock, and consequently transformed it, according to the above, into the triphosphate of lime, of which the *exterior layer* is composed, whilst the resulting phosphate of ammonia, on the other hand, was washed away by the rain.

It was found upon experiment that, on account of the peculiar state of aggregation of the particles of the rock, liquid ammonia had to remain for a long time in contact with it before appreciable quantities of phosphate of ammonia could be found in solution.

(3.) If extracts of both parts of the rock in water were treated with dilute alcohol, that of the *body of the rock* produced a voluminous, white precipitate of sulphate of lime, whilst that of the *exterior layer* remained clear, though it produced instantly a precipitate with chlorid of barium after having been acidulated with hydrochloric acid. This is a proof that in the *exterior layer* the sulphuric acid is present in the form of sulphate of soda, whilst the *body of the rock* contains it as sulphate of lime.

4. *Chemical Analysis and Comparison of Serpentine Marbles known under the name of Verd Antique;* by CHARLES T. JACKSON. (Read before the Boston Society of Natural History, February 20th, 1856.)—Having made the original geological surveys of the great masses of ser-

pentine marbles, which occur in the northern part of the State of Vermont, and described such as would furnish a marble identical with the celebrated verd antique of Europe, I have since been requested to institute a mineralogical and chemical comparison of the European and Vermont varieties.

The results to which I have arrived possess some scientific as well as practical interest, for they not only show a curious replacement of carbonate of magnesia for carbonate of lime, the magnesite being most abundant in the Vermont marble, while calcite is the predominant spar in the European variety. It has also been ascertained, by experiments made by me some years since, that the Vermont serpentine marble and that mixture called verd antique, are uncommonly durable, resisting not only atmospheric agencies, but also the action of acids, and to a remarkable extent that of fire.

Dr. Hayes, in an interesting report on this marble, has confirmed these results, and I am happy in being able also to verify his analysis of some of the magnesite veins, while I also add now some new analyses of other veins in the Vermont marble, and of the calcite of the European verd antique. I offer like analyses of the serpentine of the verd antique, both of Europe and of Vermont, showing their identity of composition, and also an analysis I made many years since of the softer serpentine of Lynnfield in this State.

Serpentine consists essentially of hydrous silicate of magnesia and silicate of the protoxyd of iron, with occasionally a little oxyd of chromium—these oxyds giving the green color to the serpentine. The presence of water of composition in serpentine materially affects its hardness, the softer varieties containing the largest proportion of water. In some varieties I found as much as 15 per cent, while the lowest was 7 per cent. Both the verd antique serpentine of Europe and of Roxbury, Vermont, contain between 12 and 13 per cent of water. That from Proctorsville, Vt., contains but 7 per cent, and of Roxbury 13, while that from Europe contains 12·5 per cent, and that of Lynnfield 15 per cent.

Verd antique marble may be defined to be serpentine mixed with or containing numerous veins of magnesian carbonate of lime. The relative proportions of these ingredients may vary considerably on account of the isomorphic, or rather plesiomorphic characters of the two minerals. Carbonate of the protoxyd of iron, in like manner being plesiomorphic with both carbonates of lime and of magnesia, replaces either of those minerals in all proportions, without changing the angles of the crystals more than one degree.

It will be observed on examination of the analyses I have made, that in the Vermont serpentine the white spar veins are chiefly composed of magnesite, while there are also veins consisting of magnesian carbonate of lime and of carbonate of iron. The relative proportions of these magnesian and ferrous carbonates in the Vermont marble are nearly the reverse of those in the European variety, while according with the law of isomorphous substitution of mineral ingredients.

Owing to the refractory nature of serpentine, and the difficult erosion of the magnesite, the Vermont verd antique is less liable to decomposition from atmospheric agencies, and also has the property of resisting a

high temperature, and even the action of minerals and other acids, far beyond the celebrated verd antique of Italy. When highly polished, it is a rich and beautiful green marble, veined with white, and sometimes is richly mottled with magnesite and dolomite spar. Its polished surface is not liable to erosion from atmospheric causes, and will offer no hold for lichens, mosses, or other parasitic vegetation, which so frequently mar the beauty of our more open grained white monumental marbles.

1st. Chemical analysis of the white veins of European verd antique. These veins, picked out with great care to avoid any mixture of particles of serpentine, yielded per cent—

Carbonate of lime,	81·00
Carbonate of magnesia,	11·70
Carbonate of iron,	7·30
	100·00

2d. Chemical analysis of the white veins of Roxbury, Vermont, verd antique marble. These veins were quite common in the slabs examined by me. They were picked out with care to avoid any admixture of serpentine. On analysis they yielded—

Carbonate of magnesia,	80·00
Carbonate of lime,	15·00
Carbonate of iron,	3·50
Silica and loss,	1·50
	100·00

It will be observed that the carbonate of lime in the European marble, is represented by carbonate of magnesia in the Vermont variety, and the carbonate of magnesia by carbonate of lime, a reversal of these ingredients.

3d. Chemical analysis of the magnesite veins in Roxbury, Vt., verd antique.—These veins are probably like those analyzed by Dr. Hayes. They yielded per cent—

Magnesia,	38·88
Carbonic acid,	37·12
Protoxyd of iron,	9·00
Undecomposed serpentine,	15·00
	100·00

The protoxyd of iron was originally in combination with carbonic acid in the stone, forming carbonate of iron, an isomorph with carbonate of magnesia.

4th. Chemical analysis of the dolomite spar veins in Roxbury, Vt., serpentine.—A cleavage crystal, with angles of 106·15°, was analyzed, and yielded—

Carbonic acid,	46·50
Lime,	30·52
Magnesia,	18·47
Protoxyd of iron,	4·25
Silica,	0·05
	99·79

In this mineral the carbonic acid is combined with the lime, magnesia and protoxyd of iron.

5th. Chemical analysis of the serpentine of the verd antique of Europe.—It was picked out clean as possible, reduced to small grains, and washed with very dilute muriatic acid, to cleanse it from adhering carbonate of lime. The attack was made by means of carbonate of soda, in the usual manner of rendering insoluble silicates soluble in acids. The results obtained were:

Silica,	42·40
Magnesia,	31·20
Protoxyd of iron,	13·90
Water,	12·50
	100·00

The Roxbury, Vermont, serpentine, analyzed in the same manner, yielded:

Silica,	42·60
Magnesia,	35·50
Protoxyd of iron and ox. chromium,	8·30
Carbonate of lime,	0·60
Water,	13·00
	100·00

Chemical analysis of serpentine from Lynnfield, Mass.—a light green and rather soft variety.

Silica,	37·5
Magnesia,	41·0
Protoxyd of iron,	2·5
Carbonate of lime,	4·0
Water,	15·0
	99·0

This variety of serpentine is capable of being decomposed by boiling sulphuric acid, and was at one time used in the manufacture of sulphate of magnesia.

It is too soft to be used for ornamental marble, but it withstands heat perfectly after it has been gradually baked, so as to expel its water of composition.

It comes nearer to the precious or noble serpentine in composition than to that of the serpentines of verd antique marble, which are much harder than noble serpentine.

III. BOTANY AND ZOOLOGY.

1. *DeCandolle's Prodromus*, vol. xiv, Part I. 1856. pp. 492.—We are grateful to M. DeCandolle for having issued this publication without longer waiting for the large family of *Laurineæ*, still in the hands of Professor DeVriese. Only we beg, that when the concluding part of volume thirteen is given us, it may be paged continuously with the present part. For the thousands of references that will have to be made to it in botanical writings, it should suffice to cite volume and page, with-

out having to intercalate an additional numeral for the part, as in vol. xiii;—where, however, it could not be well helped, except by counting the *Solanaceæ* and *Plantaginaceæ* as a whole volume, which would have been the better way on every account.

The present issue, which has long been anxiously expected by botanists, comprises the *Erigoneæ* (115 species in seven genera) by Mr. Bentham, and the rest of the *Polygonaceæ* by Prof. Meisner, long the principal authority for this order; the *Mirysticaceæ*, by M. DeCandolle himself; the *Proteaceæ*, by Prof. Meisner; the *Penæaceæ* and the *Geissolomaceæ*, by M. DeCandolle, the last named order comprising only a single genus of a single known species. For United States botanists, therefore, the interest of the volume centers in the *Polygonaceæ*, which from the great knowledge and talent of the monographers, are doubtless admirably elaborated. To the *Eriogoneæ*, which have increased from the three species known to Pursh to 115 here described, considerable additions may already be made, from recent discoveries in our southwestern regions. A. G.

2. *Flora Vectensis: being a Systematic Description of the Phænogamous or Flowering Plants and Ferns indigenous to the Isle of Wight;* by the late William Arnold Bromfield, M.D., &c. Edited by Sir Wm. Jackson Hooker, K.H., &c., and Thomas Bell Salter, M.D. London: Pamplin, 1856, pp. 678, 8vo. Accompanied by a Botanical Topographical Map of the Isle of Wight, separately mounted.—The many American friends of the lamented Dr. Bromfield, who had the pleasure of personal intercourse with him during his visit to this country ten years ago, will be glad to know that the great work for which he had made such extensive preparation, and left unfinished at his premature death (at Damascus, in 1851), has been edited by most competent and friendly hands, and, through the devotion of a near surviving relative, is at length given to the world. Although it is not all that Dr. Bromfield would have made it had he been spared for its completion according to his plans, it is still an important contribution to general botanical science, as well as a model local Flora. The descriptions, so far as they were written out by Dr. Bromfield, were in all cases drawn from fresh specimens growing on the island; and the pages abound with critical annotations. Many of these relate to North American species observed by the author during his travels in this country. A. G.

3. *Seemann's Botany of the Voyage of the Herald;* parts 7 and 8, published together, comprise the Flora of North Western Mexico; i. e. as represented in the collections made by Lay and Collie in Beechey's voyage, by Barclay, Hinds, and Sinclair, in the voyage of the Blossom, under Capt. Belcher, and in the voyage of the Herald by Mr. Seemann himself; to which are added a few plants collected by Mr. Potts at Chihuahua. Mr. Seemann penetrated from Mazatlan to Durango, but at a late season of the year, unfavorable for botanical collections. In an Introduction, the author sketches the general features of the country, as much of it as he saw, noticing especially the native and the cultivated vegetable productions. The flora itself here extends to the *Polemoniaceæ*. The determinations, as would be expected, concern some of our

United States plants. *Clematis Pitcheri* is referred to *C. reticulata*: the flowering specimens of the two are sometimes puzzling, but the carpels are quite different. *C. lasiantha*, Nutt. is identified with *C. Peruviana*, DeC.; and *C. pauciflora*, Nutt. is suspected to be *C. hexasepala*, DeC. Nuttall's *Polygala alba* is superseded by the posterior name of *P. bicolor*, H. B. K., because "the color of the flower varies, being greenish-white, white, rose-color, and dark-purple;" but for the same reason the name of *bicolor* is almost as objectionable. *C. asperuloides*, H. B. K. would be a better name to adopt, if it really belongs here, which is doubtful. *P. Americana* is here supposed to include *P. grandiflora*, Walt., *ovatifolia* and *puberula*, Gray, and *ovalifolia*, DeC., besides two or three more like it. *P. leptocaulis*, Torr. and Gr. is said to be *P. verticillata!* *Rubus Neo-Mexicanus*, Gray, is referred to *R. trilobus*, Moc. and Sesse, perhaps with reason. The *Cactaceæ* are elaborated by Mr. Scheer, from inadequate materials, chiefly supplied by Mr. Potts of Chihuahua. A considerable number of new species are characterized; all of which will have to be collated with Dr. Engelmann's recent detailed monograph. *Philadelphus Mexicanus*, Schlect, is said to embrace *P. serpyllifolius*, Gray. The *Compositæ* are elaborated by Dr. Schultz, who makes this vast family a special study. He has overlooked the determination of *Clavigera dentata*, DeC. (in Pl. Wright, 1, p. 83), and affixed that specific name to a wrong species. Dr. Schultz reduces *Delucia* and even *Cosmos* to *Bidens*; and, if formerly inclined to undue multiplication of genera, now makes, perhaps, more than amends, by not only acceding to the reduction of *Acourtia*, *Dumerilia*, &c. to *Perezia* (as proposed by the writer), but even referring the whole to *Trixis*, which is rather more than we counted on, although perhaps a logical consequence: also by reducing *Chaptalia*, *Leria*, *Lieberkuhnia*, *Loxodon*, *Oxydon*, &c. to *Gerbera!* The plates of this fasciculus which belong to the Mexican flora are three, viz.: two new species of "*Viscum*," both *Phoradendra*, and *Galactia marginalis*, Benth., which as figured looks as unlike that species as possible. A. G.

4. *Synopsis of the Cactaceæ of the United States and Adjacent Regions;* by GEORGE ENGELMANN, M.D. (From the Proceedings of the American Academy of Arts and Sciences, vol. iii), Cambridge, 1856. pp. 58.—Besides his ample and fully illustrated memoirs on *Cactaceæ*, in one of the Pacific Railroad Survey Reports, and in the Report of the Mexican Boundary Commission, still unpublished, Dr. Engelmann has prepared this general synopsis, giving a connected and systematic view of all our species, now so numerous, with full characters of those which he has not before well described. Altogether, these memoirs constitute a very full and invaluable elaboration of an extremely difficult, but very interesting family,—difficult not only from the peculiar obstacles to collecting and preserving adequate specimens of these unwieldy and uncomfortable plants, but likewise from the great uncertainty as to what are reliable characters. Linnæus knew only one Cactus of our region. Nuttall about forty years ago added four species, from the Upper Missouri, and, much later, one from California. Now, Dr. Engelmann enumerates 117 North American species, besides some extra-limital ones, holding out, however,

some hopes that, when better known, they may have to be reduced to not less than 67 species! As to geographical distribution, the author assigns them to eight separate regions, as follows:—

(1.) *The Atlantic region*, which has only one common indigeneous species; and one more from the south has just come to light.

(2.) *The Mississippi region*, with one species.

(3.) *The Missouri region*, with five species.

(4.) *The Texan region*, with twenty species, fourteen of them peculiar.

(5.) *The New Mexican region*, with sixty-six species, forty-six peculiar.

(6.) *The Gila region*, with thirty-six species, thirty of them peculiar.

(7.) *The Californian region*, with six species, five of them peculiar.

(8.) *The Northwestern region* (North California, Utah, and Oregon), with one species, and some indications of others.

This indefatigable botanist has also nearly completed a monograph of the North American species of *Euphorbia;* which however, may not be published until after his return from Europe, where he is now occupying himself with various important botanical investigations. A. G.

5. *The Musci and Hepaticæ of the United States, east of the Mississippi river;* by WILLIAM S. SULLIVANT. New York: George P. Putnam & Co. 1856. 8vo, with 8 copper-plates.—All who are now interested in the study of our *Mosses* and *Liverworts*, and the many, who, attracted by such an able guide, will yet enter this pleasant domain of botanical science, owe a large debt of gratitude to the author of this beautiful volume. Published as a contribution to the second edition of Dr. Gray's excellent Manual of the Botany of the Northern United States, it appears also simultaneously in a separate form. It grew out of an elaboration of these Cryptogamous Orders, furnished by the same skillful hand for the first edition of the Manual; but so numerous and important are the changes made, that it must be regarded as a new work. Its geographical range is judiciously extended to the whole region lying east of the Mississippi; a multitude of new and newly discovered species, both from the Northern and Southern States, have been added; the old genera and species have been thoroughly and critically revised, and brought up fully to the present Bryological standard; but, above all, eight tables of copper plates, crowded with figures in illustration of the genera, greatly enhance its value. These illustrations, from the pencil of Mr. Sullivant himself, are worthy of the highest praise, as well for the beauty as the fidelity of their execution. The engraving also is unsurpassed if not unequalled by any work of this kind. Looking at the superb volume before us, we scarcely know which to admire most, the scientific skill and marks of labor visible on every page, or the noble generosity which places within the reach of the lovers of Mosses, such a guide, at a price which must fall far short of covering the expenses incurred. Of minds and hearts like these the nation has just reason to be proud. From this point we date a new era in the study of American Bryology; and when Prof. Tuckerman does the like for our *Lichenes*, Dr. Curtis for our *Fungi*, and Prof. Harvey for our *Algæ*, the entire field of our lower Cryptogamia will be fairly open to the adventurous seeker after knowledge. T. C. P.

6. *On the probable Origin of the Organized Beings now living in the Azores, Madeira, and the Canaries;* by OSWALD HEER, (in a letter to A. DECANDOLLE).—In your Geography of Plants you have adopted the opinion of Edward Forbes, that in the Miocene period the European continent extended to the Azores and Canaries, and supported it by fresh proofs.* In fact, the predominant European character of these Islands, which occurs in their insects as well as in their flora, proves that they were anciently joined to the continent. Nevertheless we must not forget that, as compared with Europe, these islands are very different from those of the Mediterranean. They are distinguished in the first place by a much greater number of peculiar species, which constitute a third or a fifth of the plants; and in the second by some American types, which make their appearance in all these islands. There are not only certain American species which might have reached them accidentally by the agency of the winds and currents, or of man, but American genera which are represented by peculiar species. I will instance the genera *Clethra*, *Bystrobogon*, and *Cedronella*, as also the unique pine of the Canaries (*Pinus canariensis*, Sm.), which belongs to the American forms with acicular ternate leaves. The relations of the laurels is very remarkable in this respect; they form a great part of the forests of Madeira and the Canaries, dividing into four species and playing an important part. Two species (*Oreodaphne fœtens* and *Persea indica*) are essentially American types; the third (*Phœbe Barbusana*, Webb) belongs to a genus which occurs in India and America; and the fourth (*Laurus canariensis*, Webb) corresponds with the European species. By the possession of these laurel forests the islands of the Atlantic differ greatly from the African continent, where they are entirely wanting, and approach America rather than Africa, notwithstanding the proximity of the latter.

These facts obtain great importance by the observation that the flora of the Atlantic islands has much resemblance to the Tertiary flora of Europe.

In my 'Flora Tertiaria Helvetiæ,' I have proved that a considerable number of plants of the Tertiary epoch corresponded with species peculiar to Madeira and the Canaries, in such a manner that there must be a relation between the two floras. On the other hand, our Tertiary flora indicates a great resemblance to the flora of the southern United States. Many perfectly characteristic genera, such as *Taxodium*, *Sequoia*, *Liquidambar*, *Sabal*, &c., were distributed over the whole of our tertiary country, and composed partly of species very closely allied to those which now grow in America; other genera belong equally to America and Europe (such as *Quercus*, *Corylus*, *Populus*, *Acer*, &c.), and occur in the European Tertiary epoch, composed of species corresponding with the American forms.

We find similar cases amongst the terrestrial mollusca and insects, although this is not so positive as with regard to plants.

These remarkable circumstances are explicable, if we suppose that during the Tertiary epoch a terrestial formation united the continents of Europe and America, and that this surface was extended by some projection to the Atlantic islands. A glance at the map of the depths of

* DeCandolle, Géographie Botanique raisonnée, p. 1310.

the ocean by Maury, shows that the bottom of the Atlantic forms a longitudinal valley, of which the deepest parts are between the twentieth and fortieth degrees of north latitude, nearly at an equal distance from Europe and Africa, but that on the two sides of this deep valley there is a vast *maritime plateau*, which includes the Atlantic islands, as well as the whole space between the European continent, Newfoundland, and Acadia. Beyond this space another long valley, but of less depth, takes its rise, in a direction from south to northeast between Madeira and the Azores; it loses itself close to the coast of Oporto.

If we may attribute any importance to these very general data, we must admit that during the miocene period the maritime plateau above indicated was solid ground.

This country, this ancient Atlantis, would have had the same plants as central miocene Europe, of which the remains are found in the molasse of Switzerland in such astonishing profusion, that I shall be able to give descriptions and figures of about six hundred species in my 'Flora Tertiaria.' On the coast of this country the marine shells presented a great conformity in America and Europe; and this remarkable phenomenon is still reproduced, that Europe has more littoral than deep-sea species of shells and fishes in common with America; which proves that at one period a band of firm ground must have united these two parts of the world. The Atlantic islands had already risen towards the south coasts of this continent at the diluvian period. That this country was at the bottom of the sea during the miocene epoch, is shown by the fossil shells of Porto Santo and St. Vincent in Madeira and those of the Azores; but that it had emerged at the diluvian period is proved by the terrestrial mollusca of Canical, and the fossil plants of St. Jorge in Madeira.*

The islands formed at this epoch would have received their vegetation from the Atlantis in the diluvian period, and consequently at an epoch when this continent had entered upon a new phase of development. If we suppose, that then, by a subsequent depression of the soil, the connection with America was destroyed, and subsequently that which existed with Europe, we shall obtain the elements for the explanation of the existing flora of these islands. We there find the remains of the flora of the ancient Atlantis, and in consequence many types of the Tertiary flora are retained there whilst they have disappeared in Europe. These remains, with a certain number of other species, form the peculiar plants of these isles, corresponding in part with the American species because they have issued from the same centre of formation. But it is with Europe that these islands have the most species in common, probably because their connection with this continent lasted longer.

At the diluvian period the flora of central Europe was displaced by great changes of climate (extension of glaciers, &c.); and as by the depression of the Atlantis the connection with America was destroyed, the new European vegetation could not extend on that side, but only towards the east. It is thus that the characters of the new vegetation would be explained, particularly that of the lower countries, whilst the Alps and the north have undergone less change. This also is the reason of the

* See Heer, "Ueber die fossilen Pflanzen von San Jorge in Madeira." Zurich, 1855.

great analogies which occur between the north of Europe, Asia, and America. I arrive therefore at the same conclusion with yourself as regards these latter countries, namely, that the alpine vegetation is certainly the most ancient in our country, and that subsequently when the climate became warmer, after the glacial epoch, it rose from the low countries to the mountains and Alps.—*Ann. Mag. Nat. Hist., Aug.* 1856, p. 183, *from Bibliothèque Univ. de Genève, April*, 1856, *p.* 327.

7. *On the Ruminant Quadrupeds and the Aboriginal Cattle of Britain;* by Professor Owen, F. R. S., (Proc. Roy. Inst. Great Britain, May, 1856.)—The speaker introduced the subject of the Ruminant order of quadrupeds, and the source of our domesticated species, by some general remarks upon the classification of the class *Mammalia*, and on the characters of the great natural group defined by Ray and Linnæus as the *Ungulata*, or hoofed mammalia.

These are divisible into two natural and parallel orders, having respectively the *Anoplotherium* and *Palæotherium* as their types, which genera, as far as geological researches have yet extended, were the first, or among the earliest, representatives of the *Ungulata* on this planet.

The brilliant researches by Baron Cuvier, the founder of palæontological science and the reconstructor of those primeval hoofed animals, from fragmentary fossil remains in the gypsum quarries at Montmartre, were alluded to.

Diagrams of the entire skeletons of the Anoplotherium and Palæotherium were referred to, in illustration of their dental and osteological peculiarities.

The *Anoplotherium*, with the typical dentition of

$$\textit{incisors}\ \frac{3-3}{3-3},\ \textit{canines}\ \frac{1-1}{1-1},\ \textit{premolars}\ \frac{4-4}{4-4},\ \textit{molars}\ \frac{3-3}{3-3}=44,$$

had all its teeth of the same length, and in a continuous unbroken series: this character is peculiar to man in the existing creation. The *Palæotherium*, with the same dental formula as the *Anoplotherium*, had the canines longer than the other teeth, and developed into sharp-pointed weapons; necessitating a break in the dental series to receive their summits in closing the mouth.

The anoplotherium had 19 vertebræ between the neck and sacrum, viz., 13 dorsal, and 6 lumbar. The palæotherium had 16 dorsal, and 7 lumbar vertebræ.

The anoplotherium had a femur with 2 trochanters, and the fore-part of the ankle-bone, called "astragalus," divided in 2 equal facets. Its hoofs formed a symmetrical pair on each foot. Cuvier has very justly inferred that its stomach must have been complex, and probably, in some respects, like that of the camel or peccari. The palæotherium had a femur with 3 trochanters, an astragalus with its fore-part unequally divided, and hoofs, 3 in number on each foot. It most probably had a simple stomach, like that of the tapir and rhinoceros, which, amongst existing animals, most nearly resemble that extinct primitive hoofed quadruped, with toes in uneven number.

Every species of ungulate mammal with an uneven number of hoofs or toes, that has been introduced into this planet since the Eocene Tertiary

period, whether it have 1 hoof on each foot, as in the horse, 3 as in the rhinoceros, or 5 as in the elephant, resembles the palæotherium in having more than 19 dorso-lumbar vertebræ, which vertebræ also differ in number in different genera; 22, *e. g.* in the rhinoceros, 23 in the mastodon, 27 in the hyrax. The typical pachyderms, with an odd number of hoofs, have also 3 trochanters on the femur, the fore-part of the astragalus unequally divided, and the pattern of the grinding surface of the molar teeth unsymmetrical, and usually crossed by oblique enamel ridges. All the existing odd-toed or perissodactyle mammals have a simple stomach, and a vast and complex cæcum; the horned species have either a single horn, or two odd horns, one behind the other on the middle line of the head, as *e. g.*, in the one-horned and two-horned rhinoceroses.

Every species of ungulate animal with hoofs in even number, whether 2 on each foot, as in the giraffe and camel, or 4 on each foot, as in the hippopotamus, resembles the anoplotherium in having 19 dorso-lumbar vertebræ, neither more nor less; in having 2 trochanters on the femur, in having the fore-part of the astragalus equally divided, and in having the pattern of the grinding surface of the molar teeth more or less symmetrical. The horned species have the horns in 1 pair, or 2 pairs. All have the stomach more or less complex, and the cæcum small and simple. In the hog the gastric complexity is least displayed; but in the peccari the stomach has 3 compartments; and in the hippopotamus it is still more complex. But the most complex and peculiar form of stomach is that which enables the animal to "chew the cud," or submit the aliment to a second mastication, characteristic of the large group of even-hoofed *Ungulata*, called "*Ruminantia*."

These timid quadrupeds have many natural enemies; and if they had been compelled to submit each mouthful of grass to the full extent of mastication which its digestion requires, before it was swallowed, the grazing ruminant would have been exposed a long time in the open prairie or savannah, before it had filled its stomach. Its chances of escaping a carnivorous enemy would have been in a like degree diminished. But by the peculiar structure of the ruminating stomach, the grass can be swallowed as quickly as it is cropped, and be stowed away in a large accessory receptacle, called the "rumen," or first cavity of the stomach; and this bag being filled, the ruminant can retreat to the covert, and lie down in a safe hiding-place to remasticate its food at leisure.

The modifications of the dentition, æsophagus, and stomach, by which the digestion in the Ruminantia is carried out, were described and illustrated by diagrams.

The speaker next treated of the various kinds of horns and antlers; the manner of growth, shedding, renewal, and annual modifications of the deciduous horns, the peculiarities of the persistent horns, the mechanism of the cloven foot; and the provision for maintaining the hoofs in a healthy condition, were pointed out.

The following were the chief varieties of the ruminating stomach. In the small musk-deer (*Tragulus*), there are three cavities, with a small intercommunication canal between the second and last cavity; the "psalterium" or third cavity, in the normal ruminating stomach, being absent. This cavity is likewise absent in the camel-tribe, which have the cells of

the second cavity greatly enlarged, and have also accessory groups of similar cells developed from the rumen, or first cavity. These cells can contain several gallons of water. The relation of this modification, and of the hump or humps on the back, to the peculiar geographical position of the camel-tribe, was pointed out.

The modifications of the ruminating stomach, the discovery of rudimentary teeth in the embryo *Ruminantia*, which teeth (upper incisors and canines) have been supposed to characterize the pachyderms; the occurrence of another alleged pachydermal character, viz., the divided metacarpus and metatarsus in the fœtus or young of all ruminants, and its persistence in the existing *Moschus aquaticus*, and in a fossil species of antelope; the absence of cotyledons in the chorion of the camel-tribe, with the retention of some incisors as well as canines in the upper jaw of that tribe; the ascertained amount of visceral and osteological conformity of the supposed circumscribed order *Ruminantia*, with the other artiodactyle (even-toed) Ungulata; above all, the number of lost links in that interesting chain which have now been restored from the ruins of former habitable surfaces of the earth—all these and other similar facts have concurred in establishing different views of the nature and value of the ruminant order from those entertained by Cuvier, and the majority of systematic naturalists up to 1840. Thus instead of viewing the *Anoplotherium* as a pachyderm, the speaker, having regard to the small size of its upper incisors and canines, to the retention of the individuality of its two chief metacarpal and metatarsal bones, and to the non-development of horns at any period of life, would regard it rather as resembling an overgrown embryo-ruminant—of a ruminant in which growth had proceeded with arrest of development. The ordinal characters of the anoplotherium are those of the *Artiodactyla*. On the other hand, instead of viewing the horse as being next of kin to the camel, or as making the transition from the pachyderms to the ruminants, the speaker had been led by considerations of its third trochanter, its astragulus, its simple stomach, and enormous sacculated cæcum, the palæotherian type of the grinding surface of the molars, and the excessive number of the dorso-lumbar vertebræ, to the conviction of the essential affinities of the *Equidæ* with other perissodactyles (odd-toed hoofed beasts).

The primitive types of both odd-toed and even-toed ungulates occur in the eocene tertiary deposits: the earliest forms of the ruminant modification of the *Artiodactyla* appear in the miocene strata. The fossil remains of the aboriginal cattle of Britain have been found in the newer pliocene strata, in drift gravels, in brick-earth deposits, and in bone caves. Two of these ancient cattle (*Bovidæ*) were of gigantic size, with immense horns; one was a true bison (*Bison priscus*), the other a true ox (*Bos primigenius*); contemporary with these was a smaller species of short-horned ox (*Bos longifrons*), and a buffalo, apparently identical in species with the Arctic musk-buffalo (*Bubalus*, or *Ovibos moschatus*).

The small ox (*Bos longifrons*), is that which the aboriginal natives of Britain would be most likely to succeed in taming. They possessed domesticated cattle (*pecora*) when Cæsar invaded Britain. The cattle of the mountain fastnesses to which the Celtic population retreated before the Romans, viz., the Welsh "runt" and Highland "kyloe," most resem-

ble in size and cranial characters the pleistocene *Bos longifrons*. Prof. Owen, therefore, regards the *Bos longifrons*, and not the gigantic *Bos primigenius*, as the source of part of our domestic cattle.

From the analogy of colonists of the present day he proceeded to argue that the Romans would import their own tamed cattle to their colonial settlements in Britain. The domesticated cattle of the Romans, Greeks, and Egyptians bore the nearest affinity to the Brahminy variety of cattle in India. As the domestic cattle imported by the Spaniards into South America have, in many localities, reverted to a wild state, so the speaker believed that the half-wild races of white cattle in Chillingham Park, and a few other preserves in Britain, were descended from introduced domesticated cattle. The size of the dew-lap, and an occasional rudiment of the hump in these white cattle, as well as the approximation to the light grey color characteristic of the Brahminy race, seemed to point to their primitive oriental source. But the speaker could not regard the pure white color as natural to a primitive wild stock of oxen. It is now maintained by careful destruction of all piebald calves that are produced by the so-preserved half-wild breeds.

If the blood of any of the aboriginal cattle, contemporary with the mammoth and hairy rhinoceros, still flowed in the veins of any of our domesticated races, he thought it would be that of the *Bos longifrons* transmitted through the short-horned or hornless varieties of the oxen of the mountains of Wales and Scotland.

In conclusion the speaker referred to the subjoined table of the classification of recent and extinct hoofed quadrupeds, as indicative of the progressive extinction of those forms of *Ungulata* least likely to be of use to man, and of the substitution of the ruminant forms, which from the perfect digestion of their food, elaborate from it the most sapid and nutritious kinds of flesh.

UNGULATA.

Typica.

ARTIODACTYLA.*		PERISSODACTYLA.†
Anoplotherium.	*Auchenia.*	Palæotherium.
Chalicotherium.	Merycotherium.	Paloplotherium.
Dichobune.	Merycopotamus.	Lophiodon.
Cainotherium.	*Hippopotamus.*	Coryphodon.
Poebrotherium.	Dichodon.	*Tapirus.*‡
Xiphodon.	Hyracotherium.	Macrauchenia.
Moschus.‡	Hyopotamus.	Hippotherium.
Antilope.	Anthracotherium.	*Equus.*
Ovis.	Hippohyus.	Elasmotherium.
Bos.	Chœropotamus.	*Hyrax.*
Cervus.	*Dicotyles.*	*Rhinoceros.*
Camelopardalis.	*Phacochœrus.*	Acerotherium.
Camelus.	*Sus.*	

* Αρτιος, *par*; δάκτυλος, *digitus.*

† Περισσοδάκτυλος, *qui digitos habet impares numero.*

‡ Only those genera printed in *italics* now exist.

Aberrantia.

TOXODONTIA.

Toxodon.
Nesodon.

PROBOSCIDIA.

Elephas.
Mastodon.
Dinotherium.

SIRENIA.

Manatus.
Halicore.
Rytina.
Halitherium.
Prorastomus.

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Notice of the Scientific Results of the Expedition to the North Pacific Ocean, under the command of Com. John Rodgers;* by W. STIMPSON. (From a letter to J. D. DANA, dated Washington, Dec. 1, 1856.)

Prof. DANA: *Dear Sir*—Our Expedition having returned, thinking you would be interested in some account of the zoological results, I transmit herewith a general view taken from the catalogues, which will give you some idea of the character and extent of what has been done. As the Expedition had for its object marine explorations and surveys only, no opportunity was afforded for inland research; so that fishes and invertebrates—the chief inhabitants of the sea—form the mass of the collections. The principal object kept in view has been, when on coasts little known zoologically, the numerical accumulation of species. This course was not only indicated by the importance of adding to our very imperfect knowledge of the geographical distribution of animals in those seas, but was, so to speak, necessitated by the shortness of our stay at the different points visited. Minute anatomical examinations are always made with difficulty on board ship, beside requiring much time; and if these be carried on during the few days spent in harbor, the collection of specimens and the investigation of the general character of the fauna of the district must be neglected; in other words, for the sake of complete examinations of two or three species, opportunity is lost for gathering and briefly noticing a large number of forms, many of which may prove to be of the greatest interest to science.

The course pursued has been this. At each place visited, daily excursions were made to such distances as were necessary for the collection of as large a number of species as could be examined and preserved during the remainder of the day. As soon as the proper amount of materials was secured, they were immediately brought on board ship and spread out for examination. The soft animals were attended to first, together with those whose structure will not allow of preservation in alcohol (as *Nudibranchiates*, *Planariæ*, etc.) These, if new species, were drawn and described in full upon the spot, whatever expense of time might be necessary, as no other means exists of preserving their forms and colors. Then the colors of those animals (as the fishes, crabs, and echinoderms) whose *forms* are easily preserved, are rapidly noted and the specimens killed and placed in their proper receptacles. In the mean time such animals as expand their tentacula, branchiæ, or other parts under favorable circumstances (as polyps, holothurians, bivalve mollusks, etc.), have

been lying quietly in the vases of sea-water allotted to each, and glanced at from time to time, that they might be drawn at the favorable moment, the animals of the rarer shells were treated in this latter manner, many remaining undisturbed for hours before they would show themselves to the anxiously watching Malacologist, and then timidly withdrawing at the least alarm.

The endeavor has always been made to secure full notes of all such external phenomena as pertain to the *living* animal, and such as cannot be observed in preserved species. Thus notes of greater or less extent, from a full description and colored drawing down to simple statements of locality, habits, and station, have been made upon over 3000 species.

By far the greater part of the animals observed during the expedition have been invertebrates. At the present day new forms of mammals and birds are rare, and only to be sought for in the interior of continents, or of the larger and less known islands, as those of Japan. As the objects of the expedition did not include land-travelling to any extent, only such kinds of the larger animals were secured as could be obtained in short excursions from the ship. In Japan, a considerable variety might doubtless have been collected, did not the laws of that country, and a stipulation in our treaty, expressly forbid "the shooting of birds and animals." Perhaps the most interesting species are those collected on the shores of Behring's Straits.

About ninety species of reptiles were brought home, one-third of which will probably prove new to science. Fishes we had always good opportunities of procuring, and specimens of more than five hundred species were collected, a large proportion of which are those smaller forms which have not yet been studied. Of the larger kinds there are several curious species not yet to be found in any of our collections.

As before said it was not to be hoped that any great number of novel forms of Vertebrata would occur, as these have naturally occupied the primary attention of former collectors; while many of the ports visited by the expedition have been long inhabited by Europeans, and repeatedly searched. With the Invertebrata the case is different. These are chiefly inhabitants of the sea, or of such places on land or freshwater as to elude all observation except such as is expressly directed to their search. In many their very minuteness enables them to escape notice. A large proportion live in the depths of the ocean, and can only be brought to light by dredging, which, in the Pacific has as yet been carried on to little extent. It is at once apparent that this is the field in which the most successful gleanings are to be made at the present day, and to this chief attention was directed.

The expedition was fortunate in having for its commander one who appreciated the value of all departments of science, even of those whose utility is not at once apparent, not forgetting natural history. To Capt. Rodgers we are indebted for the whole of the rich collection of invertebrates made in the Arctic Sea, during the month of August, 1855, the dredge being almost constantly used from the ship, although the whole of the scientific corps were then making explorations on shore at Seniavine Straits.

Generalizations, including those upon geographical distribution, for which a large amount of materials has been accumulated, cannot of course now be made or presented here, and will properly be considered and compared with former deductions after the new species have been worked up, and their bearing upon the various groups and subdivisions established.

The whole number of species collected, in all departments, is about 5300. The number of specimens may be stated approximately as 12,000. The species are distributed among the various groups as follows:

Mammalia,	31	Naked Gasteropoda,	180
Birds,	175	Testacea,	1970
Reptiles,	90	Tunicata,	40
Fishes,	550	Bryozoa,	50
Insects,	400	Holothuridæ,	70
Crustacea,	980	Echinidæ,	66
Annelides,	220	Stelleridæ,	150
Turbellaria,	96	Polypi,	90
Cephalopoda,	20	Medusæ,	30

2. *On the Meteor of July 8th;* by Prof. N. K. Davis, of Howard College, Marion, Alabama.—All the notices of this meteor I have seen omit the mention of what appears to me the most remarkable phenomenon connected with it. The meteor itself I did not see, but on the afternoon of the above date, as nearly as I can remember, about 4 o'clock (I did not note the time) a friend called me out into the middle of the street stating that he had just seen a brilliant meteor and pointed to the place over the house tops. On looking I saw a dense white cloud, which he stated had been left by the meteor, about 1° in width and 8° in length, nearly perpendicular to the horizon, and having its lower extremity elevated about 25°. Its bearing was west of northwest, I should judge farther west than the writer in the Journal makes it. This appearance struck me as remarkable and I watched it with great interest. The air being clear, its great apparent density, its sharpness of outline and its considerable elevation, deceived me at the first glance respecting its distance, and my first impulse was to mount a horse and gallop to the spot indicated by its direction; but on changing my position purposely to ascertain, I found that it had no sensible parallax which undeceived me. By information since received, I find it must have fallen somewhere in Mississippi, at least 150 miles from this place. Of course no sound was heard here. The fumes were intensely white and dense, reminding me strongly of the fumes of chlorid formed when powdered antimony is projected into a jar of chlorine. The cloud gradually took a zigzag form and the aerial currents then slowly dispersed it, though it retained its dense appearance after it had spread considerably. I have never seen a notice of a similar fact, and the question arose in my mind—may not the luminous trail left by nocturnal meteors, which all close observers will grant is *not* always the impression retained by the retina, be due to the momentary phosphorescence of similar fumes? I say 'fumes' because I suppose the cloud to be a product of combustion in the air; but what this product may be, from its appearance alone I should be at loss to determine.

3. *Credit to whom credit is due;* extract from an Address before the American Institute, New York City, by Prof. A. D. Bache.—I wish I could so speak for the Continent, and especially for France. Since the wane of that great light of the French Academy, Arago, American scientists have had much to complain of. Since its final earthly eclipse they have more. The official publications of the doings of our real men of science are either overlooked entirely, disregarded, or named to be treated with disrespect. This, too, from those who once professed to be amongst the most devoted of the admirers of Arago, and under his lead, to cultivate friendships which might almost be termed *sentimental*, with our savants. "Write to me," said one of these distinguished men to one of our friends, "at the equinoxes and I will answer at the solstices." I wrote said the American at the equinoxes, *but the solstices* have never come. True there are cases of exception which according to the law maxim prove the rule. Not to indulge in generalities, I state, after full examination, that the methods advanced by Le Verrier, a man who of many, has no need to slight the claims of others, for determining differences of longitude by the telegraph, are but the reproduction of those used in the Coast Survey of the United States for now these eight years, the fruits of the labors and studies of Walker and Loomis, Gould and others. Neither the method coincidences which he lauds, nor that of signalizing the transit of stars, which he considers of the highest merit, *are new*, but have been practiced for years, and every few months have been published over and over in official reports, in the proceedings of recognized scientific bodies, and constitute in part, what may properly be called the American method of telegraphic longitudes. The Astronomer Royal of Great Britain, in a far different spirit, has given to the automatic register of astronomical observations by the galvanic circuit, the title which generously recognizes our claims, and assigns the origin to the United States—in the title of American method of observation.

A lesser light too, of the old world, Wychmann, of Königsberg, has just published an article on the difference of longitude by telegraph, stimulated by that of Le Verrier, and containing an outline of his mode of proceeding, which might almost serve as a history of the olden time methods of the coast survey.

Better things than this were to be expected from a German physicist. They of all Europeans have, in former days, been sore under the infliction of the egotism or neglect of the French physicists, and I remember well the unction with which the story was told me by one of these men who read all languages, that when Becquerel was reproached with his neglect of German electricians in his work on electricity, he exclaimed with a nonchalance considered typical of the Academy, "Must one know all languages to write a book?"

4. *Geographical Discoveries in Africa;* by Rev. Mr. Livingston, (Letter dated "River of the Bashukulompo, near its confluence with the Zambesi, latitude 15° 47′, longitude 28° 50′ east, 20th December, 1855.) Cong. Jour., Sept. 26, 1856.—A little leisure obtained, together with a bountiful supply of pork for my party, enables me to commence an epistle to my Yankee brother. We number 115 in all, so you may wonder whether we have a Cincinnati in intertropical Africa. We got a hippo-

potamus last night, and some elephants appearing this morning, the men ran off, and soon killed a fine cow with their spears. The flesh of the river horse is very like pork, and much esteemed in the colony as such. My men are now all cutting it up into long strips for drying, roasting it, and boiling it, and laughing. I am sitting on some grass in the midst of ranges of beautiful tree-covered hills, and after this introduction will proceed as follows.

We came down the river from Sesheki convoyed by Sekeletu and principal men, with about 200 followers. About ten miles below the confluence of the Chobe, the rapids began, which compelled us to leave the canoes, and march along the bank on foot. Twenty miles brought us to the island of Sekote or Kalai. As it was necessary to turn off to the northeast from this point, in order to avoid Tsetse, I took a canoe and went about eight miles farther down to see the Falls of Mosioatunya. When five or six miles distant, we saw five columns of smoke ascending *apparently* to the clouds. Taking a little light canoe, when about a mile above the spot, and men well acquainted with the rapids, we went to an island situated about the middle of the ledge over which the Zambesi rolls, and then crawling to the edge, peered over into the wonderful abyss which constitutes Mosioatunya (smoke sounds).

"There is always something new from Africa," said Scipio, or somebody as wise. You may see your big Niagara, but you cannot see a river leaping into a straight jacket. Imagine the Thames filled with low tree-covered hills, 300 feet high from the tunnel down to Gravesend, and its bottom formed of basalt instead of mud. Then fancy farther, a rent made in the bed from bank to bank, down through the roof of the tunnel, and the pathway to be about 100 feet below instead of what it is, the lips of the fissure being from 50 to 100 feet apart, and the whole mass of water flowing southerly, leaping down into this, and suddenly compressed from a thousand yards into fifteen or twenty, then compelled to flow from east to west, or from the right to the left bank, there turning a corner, it resumes in the fissure prolonged in that direction, its southerly course. The fissure in passing through the hills for about thirty miles, becomes much deeper, probably 300 feet. Then the river opens out again, and our goodly Zambesi flows placidly away to the northeast.

In looking down into the fissure on the right, one sees nothing but a dense white cloud with two rainbows on it. It was near midday when we visited it, and the southern declination of the sun nearly equal to the latitude. An amount of vapor rushes up which I never saw equalled anywhere else. Rising about 300 feet it becomes black, and descends in a smart shower, which soon wet us to the skin. In the distance it resembles African grass burning. We have no idea of the depth of the fissure on the right side. On the left, a large piece has fallen in, and that seems about 100 feet from the lip over which we are looking. The lip over which the water flows has its edge worn about three feet down at the three portions into which the water divides itself at low water.

At this time the falls are about 600 yards broad. There are two smaller ones, hence the five columns of vapor. This lip may be said to be serrated on the edge, several large pieces having fallen in, and it is from 50 to 100 feet distant from the opposite one, which has a clean

sharp edge; both are quite perpendicular. The opposite lip is ornamented with a large straight hedge of evergreen trees, whose leaves are constantly wet by the perpetual shower. Little streams run down from the hedge into the gulf, but never reach the bottom; the ascending mass of vapor blows them all aloft again.

In former times, the three principal falls were places of worship for three chiefs who lived in the neighborhood. I suppose they thought the feelings of awe which the scene inspires were appropriate in praying to the Barimo (gods, or departed spirits). The never ceasing roar might convey an idea of the flood gushing forth from beneath the footstool of the Eternal, and the bright rainbows immovable on the fearful turmoil below, that of Deity presiding over all unstable things, Himself alone unchangeable. But they never knew him as we do, a God of benevolence and love. They were a bloody, imperious crew. In all their villages are seen numerous human heads mounted on poles. I counted between fifty and sixty so exhibited in one village. On asking the son of the head man, who had killed the owners, what had been his father's motive, he replied, "to show strangers his fierceness." But many of these were mere boys, and he was not ashamed to own it. They had been returning, half famished, from an unsuccessful expedition against Sebituane, and this was the hospitality they met with.

I returned next day with Sekeletu on a little speculation of my own. The island on which we stood in the middle of the falls, is covered with trees which are nourished by occasional puffs of the wind carrying a gentle shower from the columns over the island. I have often planted fruit tree stones, but the climate, though much more humid than the south, is troubled with intermittent droughts, which destroy tender plants. I always lost my plants by my friends forgetting to supply moisture. Seeing it clear, therefore, that Mosioatunya would not forget to throw up vapor, nor the winds to blow, I made a little nursery for peaches, apricots and coffee, at a part of the island which I think will get about a proper quantity of the condensed vapor. The only enemy I have to fear is the hippopotamus, of which we saw footprints on the island. But a Makololo promised to make a hedge, and if the brutes don't break it, I have great hopes of Mosioatunya's abilities as a nurseryman. When the river rises four or five feet the island is totally unapproachable by man, and there is a continuous fall of a thousand yards. I am, however, a miserable judge of distances on water. I showed a naval officer a space in the bay of Loanda which I thought of the same breadth as the Zambesi, and called 500 yards. He replied, "that is 900 yards;" and yesterday, I put down the river Bamuki at a broader part than the ford, as 200 yards, but having to wade two-thirds of it, I found the whole to be 300 yards. I believe no one will, in future years, say I have overstated the size of parts described.

In coming through the country of the Balonda, we passed a little lake called Dilolo. There is a river connected with it called Lotembua, which has the singular fortune of running two ways at once. The upper or northern portion of it flows northwest into the Casai; the lower or southern half runs into the Seeba, which again flows into the Zambesi. As the Casai is the main branch of the Congo or Zaire, the Lotembua pours

some of its waters into the sea on the west coast, and some into the same receptacle on the east coast, or to write more magniloquently, Lotembua divides its waters between the Atlantic and Indian oceans.

The real form of the continent is not, as was imagined, an elevated table land, with high mountains—African Cordilleras—running from north to south, but a hollow basin with an elevated ridge, distant in both cases, about 300 miles from the coast. This is clearly evident from all the sources of the Casai flowing from the western ridge towards the centre of the continent. The feeders of the Zambesi on the west, as Loete, Simah and Chobe, flow in this direction into the Zambesi or Leambye, and all the feeders of the Leambye on the eastern side, as the Loapola, River of Babisa, &c., flow westwards, or towards the centre of the basin. Casai and Leambye run north and south respectively, till they find openings in the ridges, one by fissure (see Tuckey) and the other by Mosioatunya (see above). Lotembua is about the highest point in the basin, hence it runs both ways. The courses of all the extra basin rivers, as Couza, &c., are necessarily short. Now take care to imbibe the real idea I mean to convey, for I have more than the philosophy of the thing in view. The middle of the continent is without doubt a basin, but only with respect to the longitudinal ridges named, and *not as regards the level of the sea.*

Lake Ngami is the lowest spot in it I have visited. Water boils there at 206½°, (this is from memory,) and it therefore is a little more than 2,000 feet; Linyanti, 205½°, Lotembu, the highest part, 203°, the top of the ridges on both sides, 202°. Not having a table by me, I can only conjecture, from recollection, that this indicates between 3,000 and 4,000 feet above the level of the sea. When we came to the high land overlooking the Quango and Cassenge, I thought we had been traveling on elevated table land; but we were just on the ridge (202°) and the opposite side, which, in ascending, seems higher, is actually much lower (206°). The denudation has been more vigorously executed below it than below the eastern ascent.

In relying on the boiling point of water, I am quite aware it is not a very precise way of measuring altitudes, still, no one will despise the results, when corroborated by the general courses of the rivers, and the general direction of the ranges of hills north-northwest and south-southeast. Such, too, is the strike of the strata, and the dip is toward the centre of the continent. These rocks are of the series which form the bottom of the oldest silurian, sometimes of sandstone, with banks of shingle, and not a trace of vegetable existence; others of mud, now schist, but nothing impressed on it but the ripple mark of the hot primeval ocean; and at other times of sandstone schist, containing impressions of Algæ, and other of the lowest forms of vegetable existence. Pieces of the last have often been brought up by eruptive rocks, which have partially elevated and filled portions of the basin. Without dosing you *ad nauseam* with geology, for the pursuit of which science I have myself always had more inclination than leisure, I may add that before the fissure of Mosioatunya was made, there was a vast lake west of it, which included Lake Ngami, Libele, Linyanti, &c. &c., in its bosom. The Zambesi then flowed in a bed on the left of the fissure, and in which a small

stream called Lekono now flows, but it runs away back and joins the river above the falls. The beginning of the ancient bed is on the same level as Linyanti. Leaving it, and going northeastward, we come to another called Ungnesa, which also turns backward and joins the river above the rapids. On the centre of the ridge runs the Kalomu—scarcely more than mountain torrents all of them—but it flows south and joins the Zambesi below the falls. Then the Mozuma, which is the first showing inclination eastward. The boiling point of water corresponds to 202° on the Kalomo. This ridge is perfectly salubrious, and so is that on the west. There are no marshes on it. The grass is short, and well suited for pasturage.

It once contained a very large population, as the ruins of towns everywhere testify, and is well adapted for raising native produce. It has again and again been overrun by fire and sword. The people are humbled by these calamities, and after the first suspicions were over, received us joyfully as harbingers of peace. "Give us sleep," said they, "that we may repose without dreaming of men pursuing us with spear in hand." They are very degraded, dress *in puris naturalibus*, and make one think that man is the most inelegant animal alive. I asked an old man with as much civility as I could muster—for it is as useful among savages as among savans—if he had never thought of a slight departure from his original costume, a fig leaf, for instance, though he might not approve of "going into bags," (they call our dress so). He looked at me with that sort of leer which the so-called freethinkers adopt when pitying our weakness for believing that the Bible is verily God's message to man, and answered with a smile, "he was not troubled with my weak prejudices!"

The Makalolo once lived on the ridge, and I remember that on asking Sebituane if his country were all as unhealthy as Linyanti, he referred me to this, and told me he was forced to leave it by the Matibale. It has all the appearance of salubrity, open, undulating downs, with but few trees except on the hills, which are always covered with them. Both eastern and western ridges have the same character, indeed, many of the trees are identical with those on the slopes down from Cassenge; and so are the plants and rocks—mica slate glancing in the sun like burnished gold. They extend a long way north.

Examination of the fissure which drained such a vast extent of the basin, shows it is of no great antiquity. There are indications that the force acted also at the falls of Gonge, and drained the Borotse valley; the Roovooma, Simpopo, and other eastern rivers, were liberated in the same way, and so were the Orange and Congo. Besides banks of shells on both coasts, showing recent elevation, there are other indications in the failure of nearly all streams and fountains within the ridge whose course was to it or westwards, and if this is the geological process now going on—a draining process on a vast scale—it is, perhaps, not too speculative to think it tends to a healthy millennium for Africa. * * *

We have a very nice bean in this country called earth nut. Some were taken out at our second visit to the Lake, and Mr. Moffatt, Jr., sent a few to Natal. They are now an article of export thence. It requires only four months from planting to maturity, and as it yields beneath the soil, I think it might be acclimatized in the United States. It yields

the sweet oil of commerce by being pounded and thrown into boiling water. When roasted, and when not quite ripe, it far exceeds almonds or walnuts.

We hope to reach the coast in a month or two. It is an entirely new path; no European ever crossed the continent before. Arabs, however, have done it frequently, and it was accomplished by two native Portuguese. This fact was deemed of so much importance, it was noticed in the history of Angola. There never was any chain of stations across the continent, as mentioned by some Portuguese. Pereira's journey to Cazembe is known; he was heard of only here. Indeed the use they apply the ivory to shows they had none. The chief's grave at Kalai had seventy large tusks placed around its edges, the points looking inwards, the bodies sunk half way in the ground; there were thirty on other graves, all rotten from exposure to the weather. This was the common application among all these tribes.

5. *On Isothermal Lines;* by Prof. Hennessy, (Proc. Brit. Assoc., Aug. 1856, Ath. No. 1503.)—After some preliminary remarks as to the general influence of the distribution of land and water on the forms of isothermal lines, the author proceeded to discuss the distribution of these lines in islands. By considering an island situated so as to have its shores bathed by a warm oceanic current, if the influence of direct solar radiation be obstructed, it appears that the isothermals would be closed curves surrounding the centre of the island and having some relation to its coast line. The influence of ranges of mountains, and in general of inequalities in the surface of the island, as well as the modifying action of general winds, and the resulting changes in the shapes of the isothermals, were explained. By the introduction of solar radiation it now follows from the mathematical theory of heat that the entire quantity of heat received by a unit of surface of the island will depend on two principal terms: one, a function of the distance of the point from the coast, and capable of being expressed in some cases as a function of the difference of latitude of that point and the nearest point on the coast,—and, secondly, of a term depending on the latitude and on an elliptic function of the second order, having for its modulus the sine of the inclination of the equator to the ecliptic. It hence follows that the effect of solar radiation will be to transport the centres of all the closed isothermals towards the pole of the hemisphere in which the island is situated. Some of the lines may thus ultimately terminate at the coast with their convex sides turned towards the equator, while others may still continue as closed curves in the interior. If the influence of difference of latitude and direct solar radiation were greatly predominant compared to other causes affecting the temperature of the island, the isothermals might all terminate on the coast. If the continents may be considered as immense islands so circumstanced, they become subjects for the application of these views. Prof. Hennessy then proceeded to show that the isothermals of Ireland strictly conformed to his theory. On discussing the observations collected and arranged by Dr. Lloyd, in his "Memoir on the Meteorology of Ireland," it appears some of its isothermals are actually closed curves, while others terminate at points on the coast, the shortest being close to the equator. The physical structure of Ireland, and the difference of nearly 4° between the tem-

perature of the seas bathing its shores and the air above them, rendered it probable, *à priori*, that Ireland should present a good example for the application of the theory. From the general nature of his views, Prof. Hennessy anticipated that the discussion of observations in other islands would lead to their further confirmation,—and it would ultimately follow that not only are isothermals sinuous in their shapes and not generally parallel to the equator, but that many would be found which do not at all circumscribe the axis which joins the opposite poles of the earth.

6. *Machine for Polishing specula of reflecting Telescopes*, (Proc. Brit. Assoc. Aug. 1856 ; Ath., No. 1503.)—Dr. R. Greene exhibited a beautiful working model of a machine, invented by him, last year, for polishing the specula of reflecting telescopes. His object in constructing it was to produce a machine at the cost of 60*s.* or 70*s.* which should be equally efficacious for that purpose as a very complex machine invented by Mr. Lassell, of Liverpool, which has produced the finest telescopes ever constructed. Having accurately attained this object, the Doctor found that by adding three or four more pulleys to the machine it was capable of moving the polisher over the speculum in an almost endless variety of curves, so that the operator could choose any variety of figure he might fancy to experiment with. We have ourselves seen a great variety of those beautiful figures traced by the machine itself by fixing a black-lead pencil on the working crank. A machine costing not more than 60*s.* or 70*s.* is amply powerful for polishing a speculum of 12 or 14 inches in diameter, which it will generally finish in from four to six hours. The principle of the machine consists of a vertical shaft carrying a sliding crank and an horizontal table or chuck attached to another vertical shaft, but which, being supported by sliding collars, can have its axis moved at pleasure to any distance out of the line of direction of the axis of the crank. The table can be made to revolve from right to left, or the reverse, at pleasure, and move with various velocities. All the journals move in box-wood collars or boxes, which the Doctor finds after many years' trial to be superior to bell-metal, as not heating, soiling the oil, or working loose, and recommends them for general use in mounting every kind of machinery. He also mentioned the great advantage he derived from placing the centre of the speculum a little out of the centre of the revolving table, thus making the excentricity a variable quantity, being sometimes the *sum* and at other times the *difference* of the two excentricities of the table and of the speculum on the table. Lastly, he recommended making the polisher of the three circular pieces of light wood joined together, one in the centre and the other two at right angles to the centre-piece, in place of two pieces only as usually employed, and which are liable to warp, while three pieces will never warp with any change in the dryness or dampness of the atmosphere; and in place of forming the grooves in the pitch by indentation with the edge of a strip of thin wood, the Doctor preferred fastening small squares of thin wood to the face of the polisher, and covering them with the pitch, leaving about a quarter of an inch space between the squares.

The paper gave rise to an animated debate, in which many members joined, particularly Mr. Lassell, who highly approved of the machine and the Doctor's suggestions in using it.

In the course of the discussion upon this paper, the interest in which was much increased by Mr. Lassell's taking part in it, Prof. Stoney remarked that two of the main points now brought forward have been already published. The motion which is given to the spindle carrying the speculum in order to secure the requisite motions without complex mechanism above, and the scoring up of the polisher so that the pitch may have ample room to expand laterally without getting into ridges, were both parts of the Earl of Rosse's original invention, as published in the Philosophical Transactions for 1840. Lord Rosse not only was the first to polish large specula successfully by machinery, but further pointed out in the clearest manner the principles which should be kept in view in contriving other machines to effect the same result.

7. *Observations with the Aneroid Metallique and Thermometer during a Tour through Palestine and along the Shores of the Dead Sea;* by Mr. H. Poole, (Proc. Brit. Assoc., August, 1856; Ath. No. 1504.)—During a recent tour through Palestine I carried an aneroid metallique, and though I would not presume to say that the results of observations made with it are quite correct, yet as the readings in many instances are close approximations to the calculation of Lynch and other travellers, I wish to draw attention to that instrument as affording an easy mode of obtaining approximate levelings of heights in unsurveyed countries. It is light, and can be easily carried by a strap over the shoulder. From the rack-work being visible a readjustment can be easily made when required upon ascending high mountains. A table of corrections is, however, required, and which I found must be *additive* with an *increase* of temperature (being the reverse of mercurial barometers and vacuum aneroids), as indicated by the variation in the readings at different temperatures at the same localities, as recorded in the accompanying table. In Dent's Tables 85 feet are calculated for the difference of each tenth of an inch of barometer: this multiplied by 39·37 inches, equal to a metre, gives 33·46 feet, or 33·50 feet in common practice, as the multiple of each division in the aneroid metallique. In practice I found it very nearly correct:—for instance, there are 47 steps with a six-inch rise going down into the Tomb of the Virgin Mary in the Valley of Jehoshaphat, 23·5 feet; and by aneroid I read a difference of 7 millimetres × by 33·5= 23·45 feet. Again, the minaret of the Church of Ascension on the top of the Mount of Olives measured 36·5 feet; by aneroid the difference was 11 millimetres × by 33·5=36·85 feet. If the aneroid were mounted with a vernier scale, the observations could be more closely read off.

I particularly mention these comparisons of the aneroid with actual measurement, as they gave me confidence in it at the time, and also because I found on my return to London that I had arrived very nearly at the same results as Lieut. Lynch up to 2,000 feet above the level of the Mediterranean Sea, and also in the depression of the Dead Sea, 1313·5 feet by aneroid, while Lynch made it 1316·7 feet by level, and Capt. Symonds calls it 1312 feet. There is also a variation in the line of level of the Dead Sea at different seasons of the year, for I found at Ras Em Burghik three distinct lines of drift wood, one above the other,—opposite to Usdum the line of salt incrustations was 40 yards and the line of drift 70 yards distant from the edge of the sea, while along the west side of

the peninsula "El Lisan" a reef of rocks was exposed about a quarter of a mile distant from the shore, which does not appear to have been noticed by Lieut. Lynch's party,—I therefore think I must have been there when the water was unusually low. I found the temperature of the Dead Sea at the north end 82° Fahr. at 5 A. M., and 83° at the south end at 4 P. M. River Jordan and brooks on the Lisan and at the Ghor 64° each; brine spring 90°, where the small fish "Lebia" was caught close to the edge of the Dead Sea; Wady Em Burghik, temperature of water 76°; spring at Engedi 83°. At Ain Terabèh the sea was 80°, also a brine spring close to the shore, and the freshwater spring was 79°,—in the latter were a number of Lebia swimming about, the largest appearing to be about 3 inches long. A sulphurous smell was observed on passing the white hills south of Sebbeh, near Wady El Mahras, at Birket El Khalil,—but not at other places. It often blew hard during the day, but the waves never appeared to be more than 2 feet high, and the sea quickly went down after the wind ceased. Several nights were quite calm, but I never observed any phosphorescence on the water. The table of the dry and wet bulb thermometer was made by the same instrument, as unfortunately I had broken two others, and there were not any to be procured in Jerusalem. I therefore obtained the lower, or wet-bulb, temperature by wetting the bulb and waving the instrument in the shade, not having any muslin to cover it with. The relative humidity by this rough means was from 50 to 25 per cent., on the shore of the Dead Sea. The force of vapor being between 6 and 3 inches by Regnault's formula. The vapor arising from the Dead Sea when looked at from the heights of Ain jedi (Engedi) and Ghomran, had the same appearance as the fumes produced at brass castings.

8. *Geological Progress of the World;* note by J. D. DANA.—In a note to page 348 of last volume, I state that the law of progress,—"unity evolving multiplicity of parts through successive individualizations, proceeding from the more fundamental onward"—has been recognized among philosophers. It is in fact embraced in the systems of nature of the German metaphysicians; yet it is generally with them rather a vague idea, than a definitely appreciated principle. The true comprehension of the formula as it is exemplified in the Earth's physical progress, was first brought out by Prof. Arnold Guyot; and an exposition of it from several points of view is presented in his admirable work on Physical Geography entitled "Earth and Man". He has found it a fruitful principle also in the study of human history.

9. *Anatomical models by H. D. Schmidt, Philadelphia.*—In a recent visit to Philadelphia, we accompanied Prof. Leidy to examine the anatomical museum of the University of Pennsylvania. Among many rich objects of interest we observed a number of large anatomical models, of great beauty, which were made at the University by H. D. Schmidt. The models are constructed after nature under the inspection of Prof. Leidy, and are made of leather. One is a colossal model exhibiting the distribution of the great sympathetic nerve; another exhibits the superficial nervous system; and a third presents on a grand scale the anatomy of the brain. Nothing could be better adapted to aid in the teaching of human anatomy than these models, which are superior to anything hith-

erto brought from Europe, the imitation of nature being excellent, and the models themselves light and durable. They may be had at comparatively moderate prices by applying to the artist, at the University in Philadelphia. J. D. D.

10. OBITUARY.—*Death of Prof. Nicholas M. Hentz.*—Prof. N. M. HENTZ died after a lingering illness, on the 25th November, 1856, at the residence of his son, Dr. Charles A. Hentz, at Marianna, Florida.

Prof. H. was a native of France and came to the United States many years since. He was associated with Hon. George Bancroft as a teacher in the Round Hill School at Northampton, Mass. He was subsequently engaged at Cincinnati, Ohio, and then at Chapel Hill, N. C., as professor of modern languages and belles letters. From an early period he gave much study to some departments of natural history, and devoted especial attention to the arachnida and insects. Two papers of his appeared in the first series of this Journal, viz., a Conspectus of the Spiders of North America, (vol. xxi, p. 100–109;) and a description of an American Spider constituting a new sub-genus, *Spermophora*, (vol. xli, p. 116, 117). We trust that a full account of his life and scientific labors will be published by some friend acquainted with the particulars of his history.

11. *Wm. Yarrell.*—This distinguished zoologist died on the 1st of September last at Great Yarmouth, England. He was in his seventy-third year, having been born in 1784. The two prominent works of Mr. Yarrell are "The History of British Fishes," and "The History of British Birds." He also contributed various papers to scientific periodicals and society transactions. He was buried at Bayford in Hertfordshire, the last of a family of twelve brothers and sisters. His father was a news-agent in London.

12. *Professor Bojer.*—Professor Bojer, the well-known botanist of the Mauritius, died on the 4th of June last, at the age of fifty-six. His first visit to Mauritius was made in 1820. In 1837 he published his Hortus Mauritianus; and a year before his death he became Professor of Natural Philosophy in the Royal College on that island.—*Edinb. New Phil. J.*, Oct., 1856.

13. *A Treatise on Mineralogy;* by CHARLES UPHAM SHEPARD, No. II. 451 pp., 8vo, New Haven, 1857.—This part completes the issue of the third edition of Prof. Shepard's work, the first part of which appeared in 1852. We defer to another time a farther mention of the work.

14. *Elementary Course of Geology, Mineralogy, and Physical Geography;* by Prof. DAVID T. ANSTED, M.A., F.R.S., etc., Consulting Mining Engineer, etc. 2nd edition, 606 pp. 12mo. London: 1846. John Van Voorst.—This convenient text-book commences with "Physical Geography" embracing under this head, the topics of Dynamical Geology. Part II is devoted to Mineralogy which is treated at considerable length, 120 pages being devoted to it. Part III includes "Descriptive Geology" or Rocks and their fossils, in which the author begins as he should with the earliest rocks; and Part IV treats of Practical Geology. This concluding part covers 120 pages. The work is not especially adapted to American students, yet for a text-book of the size, we know of no better in our language.

15. *Chemical and Pharmaceutical Manipulations:* A Manual of the Mechanical and Chemico-mechanical operations of the Laboratory. For the use of Chemists, Druggists, Manufacturers, Teachers, and Students. 2nd enlarged edition, by CAMPBELL MORFIT, Professor of Analytic and Applied Chemistry in the University of Maryland, and CLARENCE MORFIT, Assistant Melter and Refiner in the United States Assay Office. 626 pp., 8vo, with 537 illustrations. Philadelphia: 1857. Lindsay & Blakiston. —This large volume on chemical manipulations enters fully into the construction of laboratories, as well as apparatus and processes employed in chemistry. Its chapters treat severally of the laboratory, the balance room, the furnace room as to all its arrangements and apparatus, the operating room, the division of substances (trituration, levigation, &c. &c.), the balance, weights, weighing, determination of specific gravity (including a long table of specific gravities), measures and measuring, measurements of temperature, sources and management of heat, baths, mode of producing low temperatures, fusion, ignition, cupellation, sublimation, distillation, lutes, the barometer, solution, evaporation, crystallization, desiccation, precipitation, decantation, filtration, washing; practical relations of electricity; blowpipe manipulations, glass blowing, corks, weights and measurements. Prof. Morfit is the author of several works on chemical subjects and his books are all valuable.

16. *Miscellaneous Chemical Researches;* Inaugural Dissertation for the Degree of Doctor of Philosophy, addressed to the Philosophical Faculty of the University of Göttingen; by CHARLES F. CHANDLER, of New Bedford. Göttingen: 1856.—The researches here included are an important contribution to mineralogical science. We defer a special notice of the results to another time. Besides the analyses of minerals, there is a paper on the "oxyds of the cerium metals with a view to finding a method for their quantitive separation," and on artificial heavy spar.

17. *The Microscope and its Revelations;* by WM. B. CARPENTER, M.D., F.R.S., F.G.S., Examiner in Physiology and Comparative Anatomy in the University of London, President of the Microscopical Society of London, with an Appendix containing the Applications of the Microscope to Clinical Medicine, etc., by FRANCIS GURNEY SMITH, M.D., Professor of the Institutes of Medicine in the Medical Department of Pennsylvania College, etc. 724 pp., 8vo, illustrated by 434 engravings on wood. Philadelphia: 1856. Blanchard & Lea.—Dr. Carpenter brings to the work of writing on the microscope, a long experience with the instrument in the way of profound original research, and stands in this respect far ahead of most other writers on the subject. He is the well known author on General and Comparative Anatomy, and other related works. An Appendix by the American editor contains a brief notice of American microscopes, and also a chapter on clinical applications of the microscope mainly from the manuals of Beale and Bennet. The work enters with fullness into micoscopic forms and structures both animal and vegetable. The illustrations are numerous and excellent.

18. *The Quarterly Journal of Pure and Applied Mathematics;* edited by J. J. SYLVESTER, M.A., F.R.S., Professor of Mathematics in the Royal Military Academy, Woolwich, and N. M. FERRERS, M.A., Fellow of Gon-

ville and Caius College, Cambridge; assisted by G. G. Stokes, M.A., F.R.S., Lucasian Professor of Mathematics in the University of Cambridge, A. Cayley, M.A., F.R.S., late Fellow of Trinity College, Cambridge, and M. Hermite, Corresponding editor in Paris. London: John W. Parker & Son, West Strand. Each number 5*s*.—This able quarterly, edited by some of the best mathematicians of England, commenced in 1855.

19. *Patent Office Report for* 1855; Charles Mason, Commissioner of Patents. Vol. I, Agriculture; 488 pp., 8vo, with many illustrations. Vol. II, Mechanics; 380 pp., with 348 plates.—The first volume besides much valuable information in various departments of agriculture, contains papers of considerable length on Insects frequenting the Cotton plant, and the Climatology of the Cotton districts of the globe. There is also a report by D. J. Brown on the seeds and cuttings recently obtained by the Patent Office, with suggestions as to the expediency of introducing others.

The part on Mechanics, by the publication of plates of woodcuts of the inventions patented, has become a work of very great value to the land. Three hundred and forty-eight pages are made up of closely crowded woodcuts of this kind, arranged under twenty-one heads, commencing with Agriculture 33 plates, Metallurgy 24 plates, Fibrous and Textile Manufactures 37 plates, Chemical Processes, and so on.

20. *The Illustrated Annual Register of Rural Affairs and Cultivator Almanac for* 1857, containing practical suggestions for the Farmer and Horticulturist, embellished with one hundred and fifty engravings, including Houses, Farm Buildings, Implements, Domestic Animals, Fruit, Flowers, &c.; by J. J. Thomas, author of the "American Fruit Culturist," &c. 140 pp., 12mo. Albany: Luther Tucker & Son.

21. *United States Japan Expedition*, Volume III: Observations on the Zodiacal Light, from April 2, 1853, to April 22, 1855, made chiefly on board the U. S. steam frigate Mississippi, during her late cruise in eastern seas, and her voyage homeward, with conclusions from the data thus obtained; by George Jones, A.M., Chaplain U. S. Navy. xliii pages of text and 703 plates.—The labors of the Rev. Mr. Jones on the zodiacal light have already been mentioned in this Journal. This volume contains the record of his observations—a vast collection, and a rich contribution to astronomical science. We propose to notice the work more at length at another time. The author, in his zeal for the department of science he has so much advanced, has gone to the mountains of Quito, South America, to continue his observations, where he proposes to remain for a year.

22. *First Report on the Noxious, Beneficial, and other Insects of the State of New York:* made to the State Agricultural Society, pursuant to an appropriation for this purpose from the Legislature of the State; by Asa Fitch, M.D., Entomologist of the N. Y. State Agricultural Society, Member of the Entomological Society of France, &c. 180 pp., 8vo. Albany.

23. *Smithsonian Contributions to Knowledge*, Vol. VIII. 1856.—This volume contains: Article 1st, Introduction. Article 2nd, Archæology of the United States, or sketches historical and bibliographical, of the pro-

gress of information and opinion respecting Vestiges of Antiquity in the United States. Article 3rd, On the recent secular period of the Aurora Borealis, by Prof. Denison Olmsted. Article 4th, The Tangencies of Circles and of Spheres, by Benj. Alvord, Major, United States Army. Article 5th, Researches, chemical and physiological, concerning certain North American Vertebrata, by Joseph Jones, M.D. Appendix, Record of Auroral Phenomena observed in the higher Northern Latitudes, compiled by Peter Force.

23. *Journal of the Academy of Natural Sciences of Philadelphia.* Vol. III, Part III.

Art. XIII.—Description of some remains of fishes from the Carboniferous and Devonian Formations of the United States. By Joseph Leidy, M.D.

Art. XIV.—Description of some remains of extinct Mammalia. By Joseph Leidy, M.D.

Art. XV.—On the Sandstone Fossils of Connecticut River. By James Deane, M.D.

Art. XVI.—Plantæ Kaneanæ Grœnlandicæ. Enumeration of Plants collected by Dr. E. K. Kane, U. S. N., in his first and second expeditions to the Polar regions, with descriptions and remarks. By Elias Durand.

Art. XVII.—A Commentary on the Synopsis Fungorum in Americâ Boreali mediâ degentium, by L. D. de Schweinitz. By the Rev. M. J. Berkeley, M.A., F.L.S., and the Rev. M. A. Curtis, F.A.A.A.S.

Art. XVIII.—Synopsis of the Melolonthidæ of the United States. By John L. LeConte.

Charles Wye Williams, Assoc. Inst. C. E.: Prize Essay on the Prevention of the Smoke Nuisance. 34 pp. large 8vo. London, 1856. The special Gold Medal was awarded for this essay, by the Society for the Encouragement of Arts, Manufactures and Commerce.

A. Delesse, Ingenieur des Mines, etc.: Matériaux de Construction de l'Exposition Universelle de 1855, 420 pp., 8vo. Paris, 1856.—A volume of much value on the various architectural materials of France and other countries, represented at the Great Exhibition at Paris, in 1855.

Compte Rendu Annuel addressé à S. Exc. M. de Brock, Ministre des Finances, par le Directeur de l'Observatoire Physique Central, A. T. Kupffer. Année 1854. 110 pp. 4to. St. Petersbourg, 1855.

Jahrbücher der k. k. Central-Anstalt für Meteorologie und Erdmagnetismus, von Karl Kreil; IV. Band., Jahrgang 1852. viii. and 400 pages 4to. Published by the Royal Academy of Sciences of Vienna. Wien, 1856.—A work of superior style and excellence.

Denkschriften der Kaiserlichen Akademie der Wissenschaften. Mathematisch-Naturwissenschaftliche Classe. 10th volume with 26 plates, and 11th volume with 61 plates. Many of the papers are geological, with full and beautiful illustrations, many of fossils. The 10th volume contains plates and description by A. E. Reuss, of a Carboniferous Crustacean, related to Erypterus and Pterygotus. He calls it *Lepidoderma Imhofi.* In the 11th volume, J. J. Heckel has a paper on Austrian fossil fish, with plates; and F. Unger one on fossil plants: there are besides many others of special interest.

Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften, Vienna—For January, February and March, 1856.—Each No. an 8vo volume of near 300 pages, finely illustrated. The February No. contains a systematic review of the birds of Northeast Africa, by Dr. Th. v. Heuglin.

Jahres-Bericht der Chemischen Technologie, von Dr. J. R. Wagner. Erster Jahrgang: 1855. Mit 65 Originalholzschnitten. 8vo. Leipzig, 1856. 508 pp.—The work should be in every University or College Library.

Verhandlungen des naturhistorischen Vereines der preussischen Rheinlande und Westphalens. 1856. Erstes Heft. Bonn.

Proceedings Boston Soc. Nat. Hist., Vol. V. Sept. 1856.—p. 386, Description of two new Argonauts and notice of the "leaf-fly" of Mauritius; *J. C. Parkinson.*—p. 391, *Plotus Anhinga* and *Tantalus Loculator* found in Illinois.—p. 391, Crystalline salt (hydrous sulphate of soda and magnesia) from South America; *N. H. Bishop:* and analysis; *A. A. Hayes.*—p. 394, Notes on the Alewive (Alosa vernalis); *J. Wyman.* Vol. VI.—p. 1, Prof. Jeffries Wyman, President of the Society. p. 2, List of Mollusca of Herkimer and Otsego Cos., N. Y.; *J. Lewis.*—p. 4, Note on the Nashville Warbler as found in Massachusetts; *Brewer.*—p. 6, Note on the Geographical Distribution of the Turtles in the United States; *Agassiz.*—p. 9, Probable viviparity of the Haddock; *Agassiz.*—p. 11, Descriptions of new shells; *A. A. Gould.*—p. 16, On the bituminous Coal formation of Elk Co., Pa.; *C. T. Jackson.*—p. 22, Note on the Process of Exuviation of the Limulus; *J. Wyman.*—On the Beryl rock of Grafton, N. H.; *F. Alger, C. T. Jackson.*—p. 24, Note on the Reproduction of Parasitic animals.—p. 27, Trilobites of Eastern Massachusetts; *W. B. Rogers.*—p. 30, On the Coal formation of Deep River, N. C.; *C. T. Jackson.*—p. 32, Chemical analysis of a variety of Agalmatolite; *C. T. Jackson.*

Proceedings Acad. Nat. Sci. Philadelphia, Vol. VII, No. IV.—p. 140, On a new genus and species of Urodela; *C. Girard.*—p. 141, Note on larva of Bufo Americanus and new species of Rana; *E. Hallowell.*—p. 144, Description of fifteen new species of exotic Melaniana; *I. Lea.*—p. 146, New species of Hyla from Georgia, with a plate; *J. LeConte.*—Note on the Reptiles in the collection of the Museum of the Academy; *E. Hallowell.*—p. 153, On new Reptiles ibid.; *E. Hallowell.* No. V.—p. 162, On a boring sponge (Cliona); *J. Leidy.*—p. 163, Notice of some remains of extinct Vertebrata; *J. Leidy.*—p. 165, Researches on Cyprinoid fishes of the U. States west of the Mississippi; *C. Girard.*—p. 217, Notice of species of the genus Salmo of authors chiefly from Oregon and California; *C. Girard.*—p. 220, On vertebrated remains of New Jersey; *J. Leidy.*—p. 221, On Reptiles in the Academy Collections; *E. Hallowell.*—p. 238, On a collection of Reptiles from Kansas and Nebraska; *E. Hallowell.*—p. 253, On birds in the Collection of the Academy; *J. Cassin.*—p. 255, On Vertebrated Remains from North Carolina.—p. 256, On some remains of Fish from Missouri; *J. Leidy.*